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American Political Science Review

Volume 90, Number 4, December 1996

Table of Contents

EDITOR'S NOTES viii

ARTICLES

Explaining Interethnic Cooperation
James D. Fearon and David D. Laitin 715

The Electoral Connection in the Chinese Countryside
Melanie Manion 736

Uncertainty, Shifting Power, and Appeasement
Robert Powell 749

Do Bills of Rights Matter? The Canadian Charter of Rights and Freedoms
Charles R. Epp 765

Partisan Effects of Voter Turnout in Senatorial and Gubernatorial Elections
Jack H. Nagel and John E. McNulty 780

Do Majority-Minority Districts Maximize Substantive Black Representation in Congress?
Charles Cameron, David Epstein, and Sharyn O'Halloran 794

Voting and Vetoing
Eyal Winter 813

RESEARCH NOTE

Myth and Reality in Business Support for Democrats and Republicans in the 1936 Presidential Election
Michael J. Webber and G. William Domhoff 824

FORUM

The European Parliament as a Conditional Agenda Setter: What Are the Conditions? A Critique of Tsebelis (1994)
Peter Moser 834

More on the European Parliament as a Conditional Agenda Setter: Response to Moser
George Tsebelis 839

The Claim of Issue Creation on the U.S. Supreme Court
Lee Epstein, Jeffrey A. Segal, and Timothy Johnson 845

Issues, Agendas, and Decision Making on the Supreme Court
Kevin T. McGuire and Barbara Palmer 853

BOOK REVIEW ESSAYS

Black Politics at the Crossroads? Or in the Cross-Hairs?
Paula D. McClain **867**
Stephen Burman. *The Black Progress Question: Explaining the African American Predicament*
Martin Carnoy. *Faded Dreams: The Politics and Economics of Race in America*
Desmond King. *Separate and Unequal: Black Americans and the US Federal Government*
Christopher Silver and John V. Moeser. *Separate City: Black Communities in the Urban South, 1940–1968*
Michael Peter Smith and Joe R. Feagin. *The Bubbling Cauldron: Race, Ethnicity, and the Urban Crisis*
Robert C. Smith. *We Have No Leaders: African Americans in the Post–Civil Rights Era*

Social Movements in Contentious Politics: A Review Article
Sidney Tarrow **874**
Charles F. Andrain and David E. Apter. *Political Protest and Social Change: Analyzing Politics*
J. Craig Jenkins and Bert Klandermans, eds. *The Politics of Social Protest: Comparative Perspectives on States and Social Movements*
Hanspeter Kriesi and others, eds. *New Social Movements in Western Europe: A Comparative Analysis*
Elizabeth J. Perry. *Shanghai on Strike: The Politics of Chinese Labor*
Charles Tilly. *Popular Contention in Great Britain, 1758–1834*
James W. White. *Ikki: Social Conflict and Political Protest in Early Modern Japan*

BOOK REVIEWS

Political Theory

Banning, Lance. *The Sacred Fire of Liberty: James Madison and the Founding of the Federal Republic* .... Vincent McGuire **884**
Barry, Brian. *Justice as Impartiality* .... Charles Larmore **884**
Benner, Erica. *Really Existing Nationalisms: A Post-Communist View from Marx and Engels*
Viroli, Maurizio. *For Love of Country: An Essay on Patriotism and Nationalism* .... David Miller **885**
Boesche, Roger. *Theories of Tyranny from Plato to Arendt* .... Larry Arnhart **887**
Bubeck, Diemut. *Care, Gender, and Justice* .... Wendy Gunther-Canada **888**
Conway, David. *Classical Liberalism: The Unvanquished Ideal.*
Machan, Tibor R., and Douglas B. Rasmussen, eds. *Liberty for the 21st Century: Contemporary Libertarian Thought.* .... James M. Glass **888**
DeLuca, Tom. *The Two Faces of Political Apathy* .... Philip Green **890**
Hindess, Barry. *Discourses of Power: From Hobbes to Foucault* .... Jacqueline Stevens **892**
Honneth, Axel. *The Struggle for Recognition: The Moral Grammar of Social Conflicts* .... Horst Mewes **892**
Jacobson, David. *Rights Across Borders: Immigration and the Decline of Citizenship*
Miller, David. *On Nationality* .... Jeff Spinner-Halev **894**
Kley, Roland. *Hayek's Social and Political Thought* .... Gordon Tullock **895**
Machan, Tibor R., and Douglas B. Rasmussen, eds. *Liberty for the 21st Century: Contemporary Libertarian Thought.* See Conway, David, above.
Miller, David. *On Nationality*. See Jacobson, David, above.
Ruderman, Anne Crippen. *The Pleasures of Virtue: Political Thought in the Novels of Jane Austen* .... Ethan Fishman **896**
Schluchter, Wolfgang. *Paradoxes of Modernity: Culture and Conduct in the Theory of Max Weber* .... Nancy L. Schwartz **896**

Simon, Julia. *Mass Enlightenment: Critical Studies in Rousseau and Diderot*
Wokler, Robert, ed. *Rousseau and Liberty* . . . . . Tracy B. Strong **897**
Smith, Tara. *Moral Rights and Political Freedom* . . . . . Jack Donnelly **898**
Thompson, Norma. *Herodotus and the Origins of the Political Community: Arion's Leap* . . . . . Clifford Orwin **899**
Van Parijs, Phillipe. *Real Freedom for All: What (If Anything) Can Justify Capitalism?* . . . . . Howard J. Sherman **900**
Villa, Dana R. *Arendt and Heidegger: The Fate of the Political* . . . . . Fred Dallmayr **901**
Viroli, Maurizio. *For Love of Country: An Essay on Patriotism and Nationalism*. See Benner, Erica, above.
Wokler, Robert, ed. *Rosseau and Liberty*. See Simon, Julia, above.
Young, James P. *Reconsidering American Liberalism: The Troubled Odyssey of the Liberal Idea* . . . . . Thomas L. Dumm **903**

American Politics

Allen, David S., and Robert Jensen, eds. *Freeing the First Amendment: Critical Perspectives on Freedom of Expression* . . . . . Donald A. Downs **904**
Cooper, Phillip J. *Battles on the Bench: Conflict Inside the Supreme Court* . . . . . Lawrence Baum **905**
de la Garza, Rodolfo O., and Louis DeSipio, eds. *Ethnic Ironies: Latino Politics in the 1992 Elections* . . . . . Roberto E. Villarreal **906**
Gedicks, Frederick Mark. *The Rhetoric of Church and State* . . . . . Stephen V. Monsma **907**
Gerhardt, Michael J. *The Federal Impeachment Process: A Constitutional and Historical Analysis* . . . . . Mark J. Rozell **908**
Gilmour, John B. *Strategic Disagreement: Stalemate in American Politics* . . . . . Sean Q. Kelly **909**
Grant, Wyn. *Autos, Smog, and Pollution Control: The Politics of Air Quality Management in California* . . . . . John J. Kirlin **910**
Himelfarb, Richard. *Catastrophic Politics: The Rise and Fall of the Medicare Catastrophic Coverage Act of 1988* . . . . . Christine L. Day **911**
Imig, Douglas. *Poverty and Power: The Political Representation of Poor Americans* . . . . . Kay Lehman Schlozman **912**
Jelen, Ted G., and Clyde Wilcox. *Public Attitudes toward Church and State* . . . . . J. Christopher Soper **913**
Klyza, Christopher McGrory. *Who Controls Public Lands?: Mining, Forestry and Grazing Policies, 1870–1990* . . . . . Robert H. Nelson **914**
MacManus, Susan A., with Patricia A. Turner. *Young v. Old: Generational Combat in the 21st Century* . . . . . William G. Mayer **915**
Marcus, George E., John L. Sullivan, Elizabeth Theiss-Morse, and Sandra L. Wood. *With Malice toward Some: How People Make Civil Liberties Judgments* . . . . . James L. Gibson **916**
Maynard-Moody, Steven. *The Dilemma of the Fetus: Fetal Research, Medical Progress, and Moral Politics* . . . . . Patricia Boling **917**
Mondak, Jeffery J. *Nothing to Read: Newspapers and Elections in a Social Experiment* . . . . . Marion R. Just **918**
Pagano, Michael A., and Ann O'M. Bowman. *Cityscapes and Capital: The Politics of Urban Development* . . . . . Richard E. DeLeon **919**
Pinello, Daniel R. *The Impact of Judicial-Selection Method on State-Supreme-Court Policy: Innovation, Reaction, and Atrophy* . . . . . Henry R. Glick **920**
Ramsay, Meredith. *Community, Culture and Economic Development: The Social Roots of Local Action* . . . . . Gerry Stoker **920**
Schultz, David A., and Christopher E. Smith. *The Jurisprudential Vision of Justice Antonin Scalia* . . . . . Richard A. Brisbin, Jr. **921**
Shamir, Ronen. *Managing Legal Uncertainty: Elite Lawyers in the New Deal* . . . . . Mark Kessler **922**
Sorenson, Leonard R. *Madison on the "General Welfare" of America: His Consistent Constitutional Vision* . . . . . George W. Carey **923**
Sunderland, Lane V. *Popular Government and the Supreme Court: Securing the Public Good and Private Rights* . . . . . Gary Jeffrey Jacobsohn **924**
Thurber, James A., and Roger H. Davidson, eds. *Remaking Congress: Change and Stability in the 1990s* . . . . . Paul J. Quirk **925**
Welsh, Wayne N. *Counties in Court: Jail Overcrowding and Court-Ordered Reform* . . . . . William A. Taggart **926**
Yinger, John. *Closed Doors, Opportunities Lost: The Continuing Costs of Housing Discrimination* . . . . . Samuel L. Myers, Jr. **927**

Comparative Politics

Agüero, Felipe. *Soldiers, Civilians, and Democracy: Post-Franco Spain in Comparative Perspective* . . . . Donald Share **928**

Badran, Margot. *Feminists, Islam, and Nation: Gender and the Making of Modern Egypt*
  Paidar, Parvin. *Women and the Political Process in Twentieth-Century Iran* . . . . Masoud Kazemzadeh **929**

Bennett, Vivienne. *The Politics of Water: Urban Protest, Gender and Power in Monterrey, Mexico* . . . . Helen Ingram **931**

Borón, Atilio A. *State, Capitalism, and Democracy in Latin America* . . . . Ronald H. Chilcote **932**

Brynen, Rex, Bahgat Korany, and Paul Noble, eds. *Political Liberalization and Democratization in the Arab World, volume 1: Theoretical Perspectives* . . . . Augustus Richard Norton **933**

Crewe, Ivor, and Anthony King. *SDP: The Birth, Life, and Death of the Social Democratic Party* . . . . David E. Butler **934**

Curry, Jane Lefwich, and Luba Fajfer, eds. *Poland's Permanent Revolution: People vs. Elites, 1956–1990* . . . . Robert Zuzowski **935**

du Toit, Pierre. *State Building and Democracy in Southern Africa: Botswana, Zimbabwe, and South Africa* . . . . James R. Scarritt **936**

Fulbrook, Mary. *Anatomy of a Dictatorship: Inside the GDR, 1949–1989* . . . . Helga A. Welsh **936**

Gilmartin, Christina Kelley. *Engendering the Chinese Revolution: Radical Women, Communist Politics, and Mass Movements in the 1920s* . . . . Jean C. Robinson **937**

Herman, Didi. *Rights of Passage: Struggles for Lesbian and Gay Legal Equality* . . . . Shane Phelan **938**

Kim, Hyung-Ki, Michio Muramatsu, and T. J. Pempel, eds. *The Japanese Civil Service and Economic Development: Catalysts of Change* . . . . Robert C. Angel **939**

Loewenhardt, John. *The Reincarnation of Russia: Struggling with the Legacy of Communism, 1990–1994* . . . . Nicolai N. Petro **940**

Luciak, Ilja A. *The Sandinista Legacy: Lessons from a Political Economy in Transition*
  Wright, Bruce E. *Theory in the Practice of the Nicaraguan Revolution* . . . . Gary Prevost **941**

Morris, Stephen D. *Political Reformism in Mexico: An Overview of Contemporary Mexican Politics* . . . . Roderic Ai Camp **942**

Norden, Deborah L. *Military Rebellion in Argentina: Between Coups and Consolidation* . . . . Gerardo L. Munck **943**

Paidar, Parvin. *Women and the Political Process in Twentieth-Century Iran*. See Badran, Margot, above.

Shih, Chih-Yu. *State and Society in China's Political Economy* . . . . Yasheng Huang **944**

Thain, Colin, and Maurice Wright. *The Treasury and Whitehall: The Planning and Control of Public Expenditures, 1976–1993* . . . . Paul Whiteley **945**

Thompson, Mark R. *The Anti-Marcos Struggle: Personalistic Rule and Democratic Transition in the Philippines* . . . . Gretchen Casper **946**

Torpey, John C. *Intellectuals, Socialism and Dissent: The East German Opposition and Its Legacy* . . . . Vladimir Tismaneanu **947**

Vowles, Jack, Peter Aimer, Helena Catt, Jim Lamare, and Raymond Miller. *Towards Consensus? The 1993 Election in New Zealand and the Transition to Proportional Representation* . . . . Stephen I. Levine **948**

Walder, Andrew G., ed. *The Waning of the Communist State: Economic Origins of Political Decline in China and Hungary* . . . . Yu-Shan Wu **949**

Woodward, Susan L. *Socialist Unemployment: The Political Economy of Yugoslavia, 1945–1990* . . . . Valerie Bunce **951**

Wright, Bruce E. *Theory in the Practice of the Nicaraguan Revolution*. See Luciak, Ilja A., above.

International Relations

Berger, Mark T. *Under Northern Eyes: Latin American Studies and U.S. Hegemony in the Americas, 1898–1990* . . . . Eldon Kenworthy **952**

Cimbala, Stephen J., ed. *Clinton and Post-Cold War Defense* . . . . Robert J. Lieber **953**

Danforth, Loring M. *The Macedonian Conflict: Ethnic Nationalism in a Transitional World* . . . . Adamantia Pollis **953**

Hanson, Stephen E., ed. *Can Europe Work? Germany and the Reconstruction of Postcommunist Societies* . . . . Daniel N. Nelson **954**

Landau, Jacob M. *Pan-Turkism: From Irredentism to Cooperation* .... Frank Tachau **955**
Lyons, Gene M., and Michael Mastanduno, eds. *Beyond Westphalia? State Sovereignty and International Intervention* .... Daniel J. Whiteneck **956**
Taylor, Bron Raymond, ed. *Ecological Resistance Movements: The Global Emergence of Radical and Popular Environmentalism* .... Robert Paehlke **957**

APSR EXTERNAL REVIEWERS, 1995–96 .... **959**

INDEX TO VOLUME 90 .... **965**

FORTHCOMING ARTICLES

Political Institutions and Satisfaction with Democracy, **Christopher J. Anderson and Christine A. Guillory**
Capabilities, Perception, and Escalation, **Bruce Bueno de Mesquita, James Morrow, and Ethan Zorick**
Legal Theory in the Collapse of Weimar: Contemporary Lessons: **David Dyzenhaus**
Unequal Participation: Democracy's Unresolved Dilemma, **Arend Lijphart**
Institutional Arrangements and the Creation of Social Capital: The Effects of Public School Choice, **Mark Schneider, Paul Teske, Melissa Marschall, Michael Mintrom, and Christine Roch**
Separation of Powers Games in the Positive Theory of Congress and Courts, **Jeffrey A. Segal**
Trends in the Partisan Composition of State Legislatures: A Response to Fiorina, **Jeffrey M. Stonecash and Anna M. Agathangelou**
Distance Versus Direction: The Illusory Defeat of the Proximity Theory of Electoral Choice, **Anders Westholm**

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Editor's Notes

Readers may have noticed that the *Review* no longer uses the blank pages between sections that, during the last few years, gave our otherwise fairly dense volumes a little "breathing room." Omitting these pages has given us additional room for text within the same page limit. In Volume 90, this new-found space has been devoted to additional information about methods used in the research reported in the *Review*, including some lengthy appendices providing mathematical proofs and information about samples and coding rules, as well as other supplementary and methodological details that should improve understanding and replicability of the research we report. We have also assigned some extra space to additional book reviews.

No use of reclaimed space, however, is more appropriate than that to which we are putting it in this issue: to acknowledge and thank the approximately 1,000 reviewers who have devoted their time and intellectual energies to the *APSR*'s peer review process this year. During 1995–96 (from August 19, 1995, through August 18, 1996), the *Review* received 533 manuscripts. I also made decisions this past year on 129 manuscripts that had been submitted to the previous editor but which were still in the review process during the editorial transition. The list of reviewers for 1995–96 that appears in this issue beginning on page 959 includes reviewers of any of these manuscripts whose reviews were received prior to August 19, 1996, the day we defined as the first of the next academic year.

Many reviewers evaluate more than one manuscript during the course of a year, some as many as four or five. In some cases, reviewers play very significant roles in refining the way authors shape or report the research that is ultimately presented in the *Review*. More than that, by their advice and recommendations, *APSR* reviewers not only affect the content of the *Review* but also, since many articles submitted but not published here go on to be published in other journals, influence and improve research throughout the discipline. (The same, of course, holds true for other journals' reviewers.)

This is also an appropriate time to recognize the continuing but also largely unsung efforts of the *APSR* Editorial Board and interns. Our reviewer selection process involves extensive consultation with the Board. *APSR* interns read and prepare written analyses of all manuscripts submitted and develop lists of up to 10 or 15 potential reviewers for each, based on databases of published books and articles, the subfield self-identifications of APSA members, and information about the specific research interests of scholars in our database. For each article, the author's abstract, intern's analysis, and list of potential reviewers are submitted to one or more Editorial Board members. Based on their extensive and expert knowledge, the Board members advise us about the appropriateness of individuals on our lists of potentials as reviewers for particular manuscripts and also make their own recommendations of still other scholars who would be good reviewers. With very few exceptions, every reviewer ultimately chosen for each manuscript has, in this manner, been approved or initially suggested by at least one member of the Board. Members of the Board also frequently review articles and advise me in a variety of other ways. Their names are not in the list of reviewers because that is intended to recognize the contributions of scholars who have no formal affiliation with or obligation to the *Review*.

Authors of both accepted and rejected papers often write to express their gratitude for the help they have received from our reviewers. Sometimes authors want to thank their reviewers by name but, in keeping with the blind peer review process, it seems to me advisable to thank them instead as a group. The enterprise of peer-reviewed scholarship depends on the willingness of scholars to participate in this way and, for their generosity toward their colleagues and the devotion to the discipline reflected in their work, the debt of gratitude owed reviewers is appropriately defined as a community, as well as an individual, one. The extremely large number of people involved in the review process makes the publication of the *APSR* truly a collective enterprise and the sixty or seventy authors whose work we publish each year are only the visible tip of this vast and interconnected network of collegial activity.

As is customary, this December issue of the *Review* also contains the annual index. The Index to Volume 90, beginning on page 965, lists alphabetically all authors and titles of published articles and authors of books reviewed. Further details on activities of the editorial office of the *APSR* for this year may be found in the Annual Report of the Editor, which appears in the December issue of *PS*.

Ada W. Finifter

INSTRUCTIONS TO CONTRIBUTORS

The *American Political Science Review* strives to publish scholarly research of exceptional merit, demonstrating the highest standards of excellence in conceptualization, exposition, methodology, and craftsmanship. Because the *Review* reaches a diverse audience of scholars and practitioners, contributors should demonstrate how their analysis illuminates a significant research problem, or answers an important research question, of general interest in political science.

Manuscripts being submitted for publication should be sent to Professor Ada W. Finifter, Editor, *American Political Science Review*, Department of Political Science, Michigan State University, 303 South Kedzie Hall, East Lansing, MI 48824-1032. Correspondence concerning manuscripts under review may be sent to the same address or, preferably, by e-mail to apsr@ssc.msu.edu.

The *Review* does not consider papers submitted to other journals or to publishers as part of a larger manuscript. Moreover, since space is at a premium, submission of articles substantially similar to articles submitted or published elsewhere, or to part of a book or other larger work, is strongly discouraged. Authors should discuss any such related publications in a cover letter to the Editor.

In general, the text portion of manuscripts should be no longer than 30 pages. Manuscripts must not be larger than 45 pages including all tables and figures, notes, references, and appendices. We prefer papers to be printed in 12 point type but we will accept submissions that use 10 point (but not smaller). In any case, there must not be more than 300 words to the page. All pages must be numbered. Manuscripts must be double-spaced, be printed on one side of the page only, and include an abstract of no more than 150 words. The *American*

Political Science Review style of embedded citations should be used, and there should be a separate list of references at the end of the article. Please do not use footnotes for simple citations. Papers not adhering to these guidelines are subject to being returned without review.

The *American Political Science Review* citation format now requires inclusion of month, season, or issue number as part of the volume and page identification. All manuscripts submitted for publication should now contain this information. For example, a reference to an article in the *Review* would be given as 89 (March):1–9, rather than 89:1–9. If a journal being cited does not use month or season, please substitute the issue number. On other questions of form, please follow the example of a recent issue of the *Review*.

For purposes of the *initial submission*, authors who wish to do so may place footnotes at the bottom of the pages instead of using endnotes and may locate tables and figures (on separate pages and only one to a page) approximately where they fall in the text. However, manuscripts invited for resubmission or accepted for publication must be submitted with endnotes, tables, and figures on separate pages at the back of the manuscript with standard indications of text placement, e.g., [Table 3 about here]. Please consider the necessity of making these changes on resubmission or acceptance of your paper in deciding how to format your initial submission.

For manuscripts that contain quantitative evidence and analysis, authors should describe their empirical procedures in sufficient detail to permit reviewers to understand and evaluate what has been done and, in the event the article is accepted for publication, to permit other scholars to carry out similar analyses on other data sets. As part of the review process, authors may be asked to submit additional documentation if procedures are not sufficiently clear.

Given the diversity of the *Review*'s readers, authors should strive for a presentation that will be understandable to as many scholars as possible consistent with the nature of their material.

Please follow these procedures for submission:

1. Submit *five* copies of manuscripts. Authors are urged to review all pages of all copies to make sure they are complete. *To comply with the* Review*'s policy of double-blind peer reviews, four of the submitted manuscripts should have no identifying references.* The first page of the four anonymous copies should contain only the title and an abstract of no more than 150 words. If it is important to the development of the paper that an author's previous publications be cited, whenever possible this should be done in a way that does not make the authorship of the submitted paper obvious.
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Manuscripts that are largely or entirely critiques or commentaries on previously published articles will be reviewed using the same general procedures as for regular articles with one exception. In addition to the usual number of reviewers, such manuscripts will also be sent to the scholar(s) whose work is being critiqued, in the same anonymous form as they are sent to other reviewers. Comments from the original author(s) to the editor will be invited as a supplement to the advice of regular reviewers. There are several purposes for this notice to the original author(s): (1) to encourage review of the details of analyses or research procedures that might occasionally escape the notice of disinterested reviewers; (2) to enable prompt publication of critiques by supplying critiqued authors with early notice of their existence and, therefore, more adequate time to reply; (3) as a courtesy to critiqued authors. Authors of critiques should therefore send as many additional copies of their manuscripts as will be required for this purpose.

No copies of submitted manuscripts will be returned.

Authors of accepted manuscripts are required to submit camera-ready copy of graphs or other types of figures. An instruction sheet will be provided.

Since submission procedures may be modified in the future, authors are encouraged to check for current instructions in the most recent issue of the *Review*.

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Explaining Interethnic Cooperation

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Though both journalists and the academic literature on ethnic conflict give the opposite impression, peaceful and even cooperative relations between ethnic groups are far more common than is large-scale violence. We seek to explain this norm of interethnic peace and how it occasionally breaks down, arguing that formal and informal institutions usually work to contain or "cauterize" disputes between individual members of different groups. Using a social matching game model, we show that local-level interethnic cooperation can be supported in essentially two ways. In spiral equilibria, *disputes between individuals are correctly expected to spiral rapidly beyond the two parties, and fear of this induces cooperation "on the equilibrium path." In* in-group policing equilibria, *individuals ignore transgressions by members of the other group, correctly expecting that the culprits will be identified and sanctioned by their own ethnic brethren. A range of examples suggests that both equilibria occur empirically and have properties expected from the theoretical analysis.*

> I grew up in a Jewish section of Flatbush that bordered an Italian neighborhood. Sometimes on our way to school, some Italian kids—nearly all of them went to parochial schools—would hassle and even attack us. Although they lived only a few blocks away, we didn't even know their names. We just called them "the St. Brennan's kids." Our parents would see our injuries and report the incidents to our school principal, who was Jewish, but from a different neighborhood. He contacted the relevant authorities at St. Brennan's, who would investigate the matter and punish the culprits. The funny thing was, no one ever seemed to think of calling the police. They were Irish.[1]
>
> —David Laitin

A compelling theory of ethnic conflict needs, first, to explain why interethnic relations are frequently characterized by a tension and mistrust which are relatively absent in intraethnic relations and which seem related to spirals of violence that mark interethnic more than intraethnic affairs. Second, for interethnic relations, a good theory of ethnic conflict should be able to explain why, despite the greater tensions, peaceful and cooperative relations are by far the more typical outcome than is large-scale violence (a claim we substantiate below). Third, a good theory should be able to explain why in some cases interethnic tensions are occasionally punctuated by spirals of violence, while in other cases the tensions exist, but interethnic disputes are more often "cauterized" short of war.

Among existing theories of ethnic conflict, accounts focusing on past tensions between groups that are memorialized in narratives of blame and threat tend to dramatic overprediction of violence.[2] Such narratives are almost always present, but large-scale interethnic violence is extremely rare. Theories that focus on how state authority prevents spirals of hostility or enables credible commitments to interethnic political bargains may also overpredict violence.[3] In those many spaces where state authority is absent or weak—for example, in many of the post-Soviet republics—interethnic relations frequently remain cooperative. Here, we seek to advance a theory that can account for ethnic violence without overpredicting its occurrence. We argue that decentralized, nonstate institutional mechanisms may often arise to mitigate problems of opportunism in interactions between individuals from different ethnic groups. Furthermore, we show how different decentralized mechanisms that work to preserve interethnic peace most of the time may have very different consequences for ethnic violence in the (relatively rare) event of breakdowns.

We should emphasize at the start that we are not offering a full causal theory of either ethnic peace or violence. We specify what we believe are important causal mechanisms that appear to have been systematically neglected in the anthropological, sociological, and political science literatures.[4] But we do not pretend that our formulation of the problem of opportunism in interethnic interactions or the decentralized institutional mechanisms we identify tell the whole causal story. A richer story would surely include those narratives of interethnic injury. It might also include the motivations stemming from indignities suffered by peoples who are considered of lower rank and who seek to overturn a rigid social ordering (Horowitz 1985); the motivations

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[1] This childhood memory of Laitin's nicely illustrates our main arguments.

[2] See, for an example, Kaplan 1993. A striking exception in the classical anthropological literature is Gluckman 1955 (138–9).

[3] See, for example, Fearon 1994 and Posen 1993. Neither of the authors argues that an absent or weakened state authority will necessarily make for conflict between groups. For Posen, conditions concerning geographic settlement patterns must be met; for Fearon, the minority must expect a permanent decline in relative military capability *and* significantly fear what the majority may do in the future. Nonetheless, both arguments may still overpredict interethnic violence; see below.

[4] Anthropologists, for instance, have spent many pages carefully describing the institutional mechanisms that regulate disputes within tribal societies but hardly any concerning disputes across groups.

stemming from the angers and frustrations that result from differential rates of economic development and career opportunities (Bates 1983, Gellner 1983); the ambitions of political entrepreneurs within an ethnic group who seek to enhance their power or influence through the political and/or military mobilization of their ethnic group (Gagnon 1994/95, Laitin 1995b); and, as a necessary condition for all ethnic violence, the availability of (usually young) thugs willing to risk their future (possibly to enhance their in-group status as worthy warriors) to become fully engaged in a culture of violence (Laitin 1995b). Finally, a thorough account of how ethnic disputes are contained would more systematically investigate the role of state policing (though we return to this mechanism in the conclusion).

The structure of the article is as follows. We first provide some evidence to support our claim that interethnic violence is the exception rather than the rule. Next, we informally sketch a theory of interethnic peace and violence that starts from individual-level problems of opportunism rather than from group-level animosities, as do existing rationalist and psychological theories of ethnic relations. We then make this argument more precise by developing a game model of everyday interactions within and across groups and by analyzing the two types of cooperative equilibria that emerge. In the fourth section we check the model's robustness and extract additional insights by considering alternative assumptions. We then consider empirical examples that suggest how the model and its conceptions correspond to real-world dilemmas and solutions to ethnic tensions. In the Conclusion, we summarize our arguments and offer suggestions on how the model may be extended.

THE RARITY OF ETHNIC VIOLENCE

A great many analysts of interethnic relations, including ourselves, agree that ethnic tensions are pervasive and commonplace. The standard view in political science, however, goes farther by suggesting that ethnic violence and active conflict also are ubiquitous. Violence is assumed to follow ethnic tensions as night follows day. For example, Daniel P. Moynihan (1993, 5) observes that "nation states no longer seem inclined to go to war with one another, but ethnic groups fight all the time." In the second sentence of his classic book on the subject, Donald Horowitz (1985, xi) writes: "By one reckoning, ethnic violence since World War II has claimed more than ten million lives, and in the last two decades ethnic conflict has become especially widespread."

This widely accepted view seems to be based on a biased selection of cases. Scholars have focused their attention overwhelmingly on cases of significant ethnic violence—they "select on the dependent variable." To demonstrate this point, we shall show that in two regions of the world viewed by many scholars as especially subject to violent ethnic strife—the post-Soviet world and Africa in the first generation after independence—cases of actual ethnic violence were vastly fewer than cases of potential ethnic violence.

For the former Soviet Union, although we have not found a database that would allow for replication, an informal survey of the interethnic scene supports this claim. Consider, first, the situation of Russians as an ethnic minority in the non-Russian republics. Of the fourteen former union republics (now independent states) excluding the Russian Federation, in only one (Moldova) has there been violent conflict between the titular nationality (the group after which the republic was named) and Russians. Within the Russian Federation, of sixteen former autonomous republics, in only two (Chechnia and Tuva) has there been sustained violence between titulars and Russians. This means that in these 30 (non-Russian) post-Soviet republics, in only three (10%) has there been Russian/titular ethnic violence.

Now consider ethnic violence involving non-Russians in the fourteen former union republics. These contain forty-five nationality groups that are neither titular nor Russian and that constitute at least 1% of the population of the republic.[5] Yet, there are only two cases of sustained communal violence between non-Russian minorities and titulars (Ossetians in Georgia and Armenians in Azerbaidjan). There are several cases of communal violence involving groups with less than 1% of the republican population (e.g., Abkhazis and Adhzeris in Georgia; Meshketian Turks in a variety of republics), but if we allowed *all* such cases, the percentage in which there was ethnic violence would be quite low. Thus, from this sample of non-Russian minorities, in only 4.4% of the potential cases (2 of 45) was there actual sustained violence with the titular group. And if we consider potential cases of ethnic violence between or among nontitulars, the rate for the 1991–95 period would be lower still. Even in the extremely unsettled post-Soviet world, while some ethnic violence has been horrible, it is far from ubiquitous.

Now consider Africa, a continent that many have described as especially prone to communal conflicts. In Table 1, we compute estimates of the ratio of actual to potential cases for four indicators of communal violence taken from the data in Morrison, Mitchell, and Paden (1989, 129): ethnic violence, "an event of short duration . . . in which two identifiable communal groups are antagonists in violence to secure some short-term goal"; irredentism, "an event in which an identifiable communal group seeks to change its political allegiance from the government of the territorial unit in which it resides to a political system . . . in which the authorities share the communal identification of the irredentist group concerned"; rebellion, "an event in which an identifiable communal group seeks by violence to gain increased autonomy from the national political authorities"; and civil war, "an event in which an identifiable communal group . . . seeks by violence to form a new political system based on boundaries of ethnic community."[6]

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[5] Groups are counted in each republic in which they appear. The data on groups are drawn from Bremmer and Taras 1993. The data on violence come from a variety of sources. It should be noted as well that violence that occurred principally during the Soviet period (e.g., against Uzbeks in Kyrgyzstan) is not counted.

[6] See Morrison, Mitchell, and Paden (1989, 129) for their complete definitions of these four types of communal violence; Part II of their book gives country-by-country codings.

TABLE 1. Estimates of Actual and Potential Communal Violence in Africa, Independence through 1979

Type of Communal Violence	Number of Incidents for All Years and Countries[a]	Country Mean of Incidents per Year[b]	Number of Potential Incidents for All Countries and Years[c]	Country Mean of Potential Incidents per Year[d]	Ratio of All Actual Incidents to All Potential Incidents[e]
Ethnic violence	20	.03	38,383	58.86	.0005
Irredentism	29	.04	18,757	26	.0015
Rebellion	27	.04	18,757	26	.0014
Civil war	52	.10	18,757	26	.0028

Source: Based on data in Morrison, Mitchell, and Paden 1989.

Note: There are 36 countries in the sample, those for which Morrison, Mitchell, and Paden (1989) report data on communal violence. See the text for summary statements of Morrison, Mitchell, and Paden's definitions of the four types of communal violence. In constructing the number of potential incidents of communal violence, we used Morrison, Mitchell, and Paden's estimates of the number of languages as a proxy for the number of ethnic groups in each country. Morrison, Mitchell, and Paden rely on diverse data sets, however, and this allows for more than one way to estimate the number of languages per country (which is inevitably a somewhat arbitrary endeavor). In constructing the table above we used the number of indigenous languages spoken in each country as given in their Table 2.3 (p. 43), which is based on the estimates of the linguist Jan Knappert (1965). For Knappert, the mean number of languages per country is 27. While these estimates have the virtue of deriving from (presumably) consistent coding criteria across countries, critics might argue that Knappert counts some dialectical variations as language differences and thereby his data may overestimate the number of "ethnic groups," if by that we mean the ethnic categories in which people place themselves and others. To be cautious, we therefore recalculated the columns in the table using Morrison, Mitchell, and Paden's lists of language groups in their country profiles. These data may have inconsistent coding criteria, but by often overaggregating ethnic groups (e.g., combining Kikuyu and Kamba in Kenya), they appear to provide a much more conservative estimate of the number of languages in each country. With this enumeration, the country mean is only 4.5 languages. Nonetheless, this underestimate produces fundamentally similar results to those presented above. Using this measure, the last column of the table would be: Ethnic violence, 0.0039; Irredentism, 0.0128; Rebellion, 0.0120; Civil war, 0.0230.

[a] For purposes of coding, actual cases of communal violence (of whatever form) that persisted for three years are counted three times, one for each year; two independent conflicts in the same year are coded as two incidents for that year.

[b] The mean for all countries of all incidents in a country divided by the number of full years since its independence through 1979. (Countries independent before 1960 are counted as if they became independent in 1960.)

[c] For irredentism, rebellion, and civil war, we estimated potential cases per year in each country as the number of ethnic groups in the country less one ($N - 1$), under the assumption that typically one group holds power while potential challengers come from all other groups (Morrison et al. 1989, 43, data on the number of language groups were used as a proxy for the number of ethnic groups in a country). We then summed across countries and years to arrive at the figures in this column. For potential cases of ethnic violence, we summed (across years and countries) a conservative estimate of the number of ethnic groups in each country engaged in regular interactions, namely, the smaller of $2N$ and $N(N - 1)/2$. If there are N groups, then the total number of dyads is $N(N - 1)/2$. We reason that in urban areas the upper bound $N(N - 1)/2$ may be approached, while in rural areas most ethnic groups are in regular contact with two or three other groups. Note that the results would barely change if we chose N as the estimate.

[d] The mean for all countries of potential incidents per year.

[e] Computed by dividing column 1 by column 3.

To avoid selection bias, we compare actual cases per year to indicators of potential cases per year, constructed as follows. For "ethnic violence," potential cases per country and year are a conservative estimate of the number of ethnic dyads in regular interaction in the country. For the other three indicators, potential cases per country and year are the number of ethnic dyads in the state that include the group holding state power (since that group cannot be irredentist, secessionist, or engaged in rebellion, as defined by Morrison, Mitchell, and Paden).

As should be astoundingly clear from Table 1, despite the conventional wisdom that ethnic violence is ubiquitous under conditions of cultural pluralism and weak states, the mean figure of actual violent communal events as a percentage of potential events hovers around zero. From around 1960 to 1979, communal violence, though horrifying, was extremely rare in Africa.

These data support our premise. There is a need in political science for a theory to account not only for occasional outbreaks of ethnic violence but also for the much more common outcome of ethnic tensions that do not lead to sustained intergroup violence.

ETHNIC CONFLICT AND THE PROBLEM OF SOCIAL ORDER

In broad terms, the large literature on ethnic groups and interethnic conflict offers two classes of theoretical arguments to explain the existence of such groups and the sources of conflict between them. Rationalist theories view ethnic groups as coalitions formed to extract material benefits from others or to defend possessions (Bates 1983, Gellner 1983, Olzak 1992). Violence may occur when one group threatens another's access to material benefits, which may take the form of state patronage, education, or control of specific labor markets. Psychological theories perceive ethnic groups as satisfying an inherent need to belong to a group and as allowing group members to maintain or enhance self-esteem. Conflict occurs when these psychological satisfactions are somehow threatened by another group or by members of one's own group (Horowitz 1985, chapter 3; Tajfel 1982).

Both the rational-coalition and psychological theories are intuitively plausible and probably help explain a range of particular cases of interethnic violence. Without wishing to dismiss either argument, we will pursue a new approach that postulates a different rationale for "groupness" and locates the problem of sustaining interethnic peace at the level of individual interactions rather than group-level grievances and animosities.

Our starting point finds the rationale for groups in the problem of social order. The motivating idea is that a great variety of human transactions and interactions involve the possibility of opportunism—self-interested behavior that has socially harmful consequences. Examples include cheating, shirking, malfeasance, fraud, exploitation, embezzlement, extortion, robbery, and rape. If unchecked by formal or informal institutions, the expectation of such opportunism leads individuals to avoid interactions or to take costly actions to protect themselves in them, making for a "society" of fear, poverty, and disorder. Solving this problem of social order means somehow making it possible for individuals to trust one another in everyday interactions that have scope for opportunism.

The solution characteristic of the modern era is Leviathan, a legal apparatus backed by an agency that aims to monopolize the means of violence. But as Hobbes himself recognized, the writ of the state can in practice extend only so far—even in well-ordered states travelers will still fear highwaymen, and people still lock their doors at night (Hobbes 1985, chapter 13). Even in the presence of a well-functioning legal apparatus, problems of opportunism still dog a tremendous range of interactions and transactions. Police do not patrol all corners at all times; the details of business and other transactions are observed only by those party to them; contracts are incomplete by necessity and sometimes by incentive; recourse to courts is costly, and the results are unpredictable; and the powers of legal discovery are limited. In practice, moreover, in many or even most places where ethnic groups intermingle, a well-functioning state and legal apparatus does not exist anyway. Instead, judges and judgments are partial or partisan, and the formal legal apparatus of the state is haphazard, weak, and corrupt. This all means that, whether the state is well developed and powerful or relatively weak, a whole host of everyday transactions and interactions will fall below or outside its ambit and are prone to problems of opportunism.[7]

The limited reach of the state opens up a space for more local and decentralized mechanisms for resolving these dilemmas. Often, these mechanisms developed long before the present-day state apparatus and persist after it appears.[8] For example, a common mechanism for defeating problems of opportunism is reputation in a frequently repeated relationship. Cooperation and trust within families are sustained in part by the expectation of many future interactions, as they are in many business and trading relationships. This type of solution is captured in schematic form by cooperation between two players in a repeated prisoner's dilemma. Here, strategies such as tit-for-tat that threaten to revert to noncooperation if the other player defects can sustain cooperation if the players are patient enough and expect a high enough likelihood of future interactions, despite the absence of any third-party enforcer like the state.[9]

While very important empirically for certain interactions, reputation within a long-term repeated relationship faces a major liability in that there are frequently significant benefits to be had from interacting or trading with numerous others. The more people with whom one interacts, the greater is the possible division of labor and gains from trade, and the greater the number of possible marriage partners, friends, contacts, entertainment opportunities, and so forth. But the greater the number of people with whom one interacts, the lower is the frequency of interaction with each individual on average, and this can undermine the two-party reputation mechanism. Thus, something more than that mechanism may be required to capture various benefits that would result from overcoming problems of opportunism in societies more "anonymous" than a family or a business partnership.[10]

Ethnic groups can provide a partial solution to this problem for their own members.[11] At bottom, the problem is informational. To support trust and cooperation among people who interact or transact relatively irregularly and infrequently, individuals need to be able to condition cooperative behavior on their partners' past behavior or history. That is, it must be possible to identify people who have cheated or exploited in the past if they are to be sanctioned and others deterred from misbehaving in the first place.[12] Thus, any formal or informal institutional practices that allow people to learn at low cost about the prior history of a potential business or social partner may help resolve the problem of social order in groups larger than a family.

Ethnic groups are frequently marked by highly developed systems of social networks that allow for cheap and rapid transmission of information about individuals and their past histories. Even if two Ukrainian émigrés in Canada have never met, they can probably learn something about the other's past in short order by "asking around."[13] In the case of relatively small groups living in close proximity, it is often the case that everyone knows everything about everybody, more or less. And insofar as members of an ethnic group tend to interact more frequently with one another than with outsiders, the simple two-party reputation mechanism may be employable within groups as well.[14]

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[7] Paradoxically, states themselves may create spaces of anarchy within hierarchy. They may do so intentionally, as when certain sorts of contract are deemed illegal (e.g., oligopolistic collusion), or unintentionally, as when making narcotics illegal creates a black market with a need for enforcement institutions (mafias). On the latter, see Gambetta 1993. In addition, it is crucially important that the state not enforce contracts among individuals to cooperate to overthrow the state; this is a necessary condition for the Leviathan solution to work. See Wagner 1993 for this point.

[8] Putnam (1993) suggests that the quality and nature of these local mechanisms are actually critical determinants of whether state institutions perform well. Greif (1989, 1994) and Milgrom, North, and Weingast (1990) have analyzed pre-state institutional mechanisms for policing trade relations.

[9] Taylor (1976) was the first to examine the implications of this mechanism for standard liberal justifications for the state. See also Axelrod 1984.

[10] This argument can be made more formally. Imagine a population of individuals who can pair off with one another in successive periods in order to play a prisoner's dilemma game. It would make sense for them to choose partners and stick to the same one "forever," since they could then use reputation in a long-term relationship to police cooperation. Yet, if the benefits of mutual cooperation increase with the average number of partners with whom both parties interact over time, then playing repeatedly with the same partner would be inefficient.

[11] Grief's (1989, 1994) work views certain "ethnic" institutions as solutions to problems of opportunism in long-distance trading relations. For an analysis of ethnic interactions that draws on some of the same concepts, although focusing more on issues of property rights, see Landa 1994.

[12] Ellison (1994) shows that in a strict sense this claim is too strong, although his example of how cooperation may be supported despite anonymity has very little if any empirical resonance.

[13] Julian Zahalak, personal communication. For anthropologists' examples drawn from a range of settings, see Colson 1974 (chapter 3, esp. pp. 54–9), who stresses the role of dense social networks and the process "whereby public opinion becomes mobilized through gossip" in the internal policing of cooperation in groups.

The fact that ethnic groups are often characterized by relatively dense social networks and low-cost access to information about the past history of individuals' behavior has an important consequence for intra- versus intergroup relations. Within groups, people who exploit the trust of others can be identified as individuals and sanctioned with relative ease by the response of the ethnic community. In game-theoretic terms, cooperation and trust can be supported within an ethnic group by punishment strategies that are conditioned on individual behavior, because the cost of obtaining information about an individual's past is low. By contrast, individual identification is harder in interactions across groups. Because social networks are less developed, it is more difficult to get information on a potential trading or social partner from "across the tracks." And if individuals are hard to identify or investigate across ethnic groups, then cooperation and trust across groups cannot be supported by punishment strategies that condition on individual behavior. For example, if you know nothing more than that the person facing you is a Serb, then you cannot condition your behavior on how the person acted in the past, but only on the fact that the other person is a Serb. Moreover, "the Serb" may have no individual reputation to worry about protecting in interactions with non-Serbs.

Relatively dense social networks and interactions within an ethnic group thus give rise to an asymmetry of information (identifying and getting information about individuals from other groups is more difficult). Any institutional regime for maintaining cooperation across groups must somehow address the problem this asymmetry poses. If one cannot be identified as an individual in some social interaction or if one can anticipate that what one does will not affect one's reputation, then one may have an incentive to exploit the other person or people involved.

Below, we use a game-theoretic model to make this informal story more precise and to explore its implications. In the model, we find two types of equilibria that support ethnic cooperation. Either (1) each group may hold all members of the other group liable for the actions of its individual members, or (2) the members of one group may simply ignore violations of trust by members of the other group, relying instead on the other group to identify and sanction the appropriate individual.

Under the first regime, the members of group A indiscriminately punish all members of group B for nasty behavior by an individual B. This is the *spiral regime*, in which individual defections trigger an escalation and complete breakdown of intergroup relations—noncooperation spreads immediately to all interactions between members of the groups. Under the second regime, *in-group policing*, individual defections do not trigger such a spiral and total breakdown. Instead, if a B exploits an A, members of group A continue cooperating with members of group B as though nothing had happened, while members of group B identify and sanction the individual who acted badly. Thus, ethnic groups cooperate to take advantage of each side's superior information about the behavior of individuals within the group, and this leads to the containment of interethnic violence.

FIGURE 1. The Prisoner's Dilemma Stage Game

	Cooperate	Defect
Cooperate	1, 1	$-\beta, \alpha$
Defect	$\alpha, -\beta$	0, 0

Notes: $\alpha > 1$, $\beta > 0$, and $(\alpha - \beta)/2 < 1$.

A MODEL OF DECENTRALIZED COOPERATION BETWEEN ETHNIC GROUPS

In this section we use a social matching game to formalize and expand on the above argument. In such a model, many individuals are randomly paired in successive periods, and each pair plays some game (typically a prisoner's dilemma, since this is a simple model of an exchange relation subject to opportunism). We begin by considering interactions within a single group in order to introduce notation and theoretical issues that arise in this class of models.[15] We then proceed to consider the case of two such groups whose members interact both internally and across groups.

Social Matching Games

Consider a group with n members represented by the set $A = \{1, 2, 3, \ldots, i, \ldots, n\}$, where i is the name of "person i," and n is an even number greater than three. The members of A will be paired randomly in successive periods, $t = 0, 1, 2, \ldots$, to play the prisoner's dilemma shown in Figure 1. Suppose that the individuals have a common per-period discount factor $\delta \in (0, 1)$. As an example of what this set-up is intended to represent, one may imagine a number of traders who go to a market each day, meet other traders, arrange or fail to arrange deals, and return the next day to interact with new partners (probably). Or one may imagine that each day people wander out into the world and have chance social encounters that may be good or bad, depending on how both parties act. "Defection" can be interpreted as an attempt to cheat or rob the other player, or in some contexts can be taken as avoiding or forgoing the interaction.

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[14] Our notion that ethnic groups are marked by high levels of interaction parallels Karl Deutsch's communications theory of the emergence of political communities. Different from Deutsch (1966, 177), who relates peaceful outcomes to shared meanings, we relate peaceful outcomes to shared information, which permits reputation-building and trust.

[15] For social matching games considered in the game-theoretic literature, see Calvert 1995; Ellison 1994; Kandori 1992; and Milgrom, North, and Weingast 1990. Our analysis owes a great deal to Kandori's work in particular.

Three general points about this class of models are worth making by way of introduction. First, the information the players receive about what happens in other pairs' interactions is important. The polar cases are (1) full information, that is, each individual observes what happens in every interaction in every period, and (2) atomized information, that is, individuals observe what happens only in the interactions in which they participate. Information matters because the less players know about a partner's history, the less they are able to condition how they act on the partner's past behavior.[16]

Second, social matching games normally have many equilibria, a feature they share with simpler repeated games. For example, the following strategy profiles can all form stable patterns of play (equilibria) in the game under the full information condition.

1. Every individual defects in every period, regardless of the past history of play. This is a Nash and subgame perfect equilibrium for any discount factor δ, and remains so even under atomized information.[17]
2. Individuals begin by cooperating, and if any individual is observed to have defected in any period, then all players switch to the "nasty" equilibrium of (1). This is a Nash and subgame perfect Nash equilibrium for large enough δ.
3. Individuals cooperate with any player who is not in "punishment phase" and defect against any player being punished. In one variant (3a), players in punishment phase always defect while being punished. In another (3b), players being punished cooperate with partners not being punished and defect when paired with other players in punishment phase. In both cases, a player enters the punishment phase for $T > 0$ periods if s/he defects against a player who is not in punishment phase. In (3b), if a player defects against a cooperator while being punished, the punishment phase is restarted. Both (3a) and (3b) can form Nash and subgame perfect Nash equilibria for sufficiently large δ and an appropriately chosen T.[18]

Under (1), society is characterized by multilateral nastiness and lack of trust, whereas in (2) and (3) social expectations give people the incentive to act cooperatively with each partner they meet. Note that while the *observed* equilibrium behavior of individuals in societies under (2) and (3) is identical—all cooperate with every partner they meet—this observed behavior is driven by very different social expectations about what would happen if anyone were to defect. Under (3), people expect that only defectors are punished while others continue cooperating; under (2), they expect that one defection leads everyone to start defecting. In game-theoretic terminology, equilibria (2) and (3) are the same on the equilibrium path (everyone cooperates each period), but they differ off the equilibrium path (in the event someone defects). Regarding interethnic relations, the intergroup cooperation we observe in different settings may be based on very different expectations and beliefs about what would happen in the event of a breakdown (off the equilibrium path).

The comparison of (1), (2), and (3) makes it clear that equilibria of social matching games can differ greatly in their normative and/or intuitive appeal. For instance, equilibrium (1) is inefficient since everyone gets zero each period when all would certainly prefer one. By contrast, (2) and (3) are efficient on the equilibrium path, since everyone is cooperating. Given our interest in whether and how ethnic cooperation across groups can be sustained, the analysis below focuses on equilibria that are efficient on the equilibrium path.

But the comparison of (2) and (3) suggests a further criterion as well—robustness. As stated, equilibrium (2) is not robust in the sense that if anyone deviates, then cooperation breaks down forever. This is a normatively unattractive feature if one allows for some "noise" in individuals' choices, that is, if we allow for occasional mistakes, misinterpretations of opponents' moves, or unobserved, random variability in payoffs from encounter to encounter. In such cases, play will eventually leave the path of cooperation, and the social equilibrium in the longer run will be that to which the system ultimately moves as a result of such random deviations. In what follows, we will consider only equilibria that ultimately return to mutual cooperation and thus are robust against such noise.

A final criterion for evaluating different equilibria is efficiency in the presence of noise. If there is no noise in the system, equilibria (2) and (3) are equally good. Everyone gets a payoff of one each period under both sets of expectations, as long as play remains on the equilibrium path. But if someone makes a mistake or a misinterpretation, then (2) is a decidely less attractive arrangement for society than is (3), since under (3) only the defector is punished, while under (2) everyone in society suffers. For just this reason, (2) may seem intuitively unnatural. If one were to accept the common presumption in economic theory that relatively efficient equilibria (and institutional arrangements) are more likely to evolve or to be chosen by planners, then (3) would be judged more likely to occur than (2).[19]

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[16] Note that under the full information condition a "group history" is commonly known, whereas under atomized information there is just a collection of privately observed "personal" histories.

[17] Loosely, in a Nash equilibrium everyone is choosing optimally, given what the other players are choosing. Subgame perfection adds the requirement that players do not anticipate others will carry out threats or promises not in their interest if they actually have to choose.

[18] Neither (2) nor (3) can be applied in the atomized information condition because both require players to condition their behavior on the past behavior of each partner. For 3a the condition on δ for subgame perfection may be very restrictive, due to a problem pointed out by Kandori (1992): If many players are in punishment phase, cooperators who meet may be tempted to cheat each other because they are not likely to meet other cooperators again for a long time. Kandori suggests a technical solution for this problem ("forgiving" old defectors when a new defection occurs), and it does not arise at all in the "renegotiation proof" punishment of (3b) (see footnote 19).

[19] There is also a more sophisticated argument for efficiency off the equilibrium path, termed "renegotiation proofness." Equilibria sustained by the threat of punishments that are costly for everyone are held to be incredible, on the ground that after a defection all will have an incentive to renegotiate immediate cooperation. To avoid this problem, in a renegotiation-proof punishment, the defector must make amends (or provide compensation) by letting nondefectors exploit him

To summarize, we will confine our attention to robust subgame perfect Nash equilibria that are efficient on the equilibrium path, and we compare equilibria according to how well they perform in the presence of small amounts of noise. This follows from our concern with exploring the formal and informal institutional mechanisms that support and maintain interethnic cooperation on a day-to-day basis.

A Social Matching Game with Two Groups

Suppose that there is a second group represented by the set B = $\{n + 1, n + 2, \ldots, 2n\}$. In each period, k individuals will be drawn at random from each group and paired for cross-group interactions, while the remaining $n - k$ in each group are paired among themselves for in-group interactions. Thus, $p = k/n$ is the probability that an individual interacts across groups in any period. We assume that k is less than $n/2$—thus, $p < 1/2$—so that in-group interactions are more common than cross-group interactions.[20]

As argued above, ethnic groups are typically marked by relatively dense social networks and low-cost access to information about other group members' behavior; it is often much easier to gain information about in-group than out-group members. While it is not literally true even in small ethnic groups that all members observe all interactions (the "full information condition"), rumor, gossip, and inquiry tend to be more developed and efficacious within than between ethnic groups. Furthermore, as we discuss below, in many places ethnic groups are delimited by a variety of institutions (churches, schools, respected elders, etc.) that certify and advertise individuals' reputations within the group.

For these reasons and as a first approximation, we will suppose that players in the model can identify coethnics as individuals in in-group interactions and, furthermore, that they know one another's history of play. By contrast, in cross-group interactions, individuals know only that they are paired with someone from the other group; they observe neither the individual identity nor personal history of an out-group partner. For example, imagine two groups living in nearby villages or city neighborhoods; members observe (or learn about) what goes on within their own community and what happens in occasional cross-group interactions. They do not observe what happens within the other ethnic community, and they do not learn the names or backgrounds of individuals whom they or their coethnics meet in cross-group interactions.

These assumptions are not intended to characterize every type of interethnic interaction, such as a durable, long-term relationship between two traders, for example, or members of different groups who live in the same village or neighborhood and interact very frequently with one another. Rather, these assumptions depict a particular class of interactions that pose a difficult problem for interethnic cooperation due to informational asymmetries and relative infrequency. Nor are we saying that there are ethnic groups whose members observe or learn about everything that takes place within the group. Rather, the assumption of full information within groups is intended as a tractable simplification of a much weaker and more plausible assumption—that in many places for many communities, individuals have *relatively* easier and lower cost access to information about coethnics than about individuals from other groups, whether due to gossip and rumor or to formal or informal in-group institutions.[21]

Formally, then, a history of play up to period t for the individuals in a group—say, A—will be a list of (1) who was paired with whom and what happened in all intra-A pairings up to period t and (2) which members of group A were paired with Bs and what happened in each period up to t. Members of group A observe neither what happens among the B players nor the identity or history of B players paired with As. A complete strategy for a player is a set of maps, each from the set of all such histories to period t into the range {C, D}, with one map for each $t \geq 0$.

The question of interest for this game (call it G) is whether and how cooperation can be supported in the *inter*ethnic interactions, which are essentially anonymous and also relatively infrequent. Anonymity implies that members of the other group cannot hold a cheater individually responsible, thus short-circuiting the mechanism used within groups to support cooperation, as in equilibrium (3) above. Infrequent interactions will tend to undermine the incentive to act nicely even if we were to assume that interethnic interactions are not anonymous.

In order for individuals to be willing to cooperate in the interethnic interactions in an equilibrium, they must face some sort of punishment (that is, loss of future payoffs) for defecting. Logically, there are three possible ways such punishment can be delivered: by members of the other group; by members of the defector's own group; or by members of both groups. We will describe strategies of the first and second types that can support interethnic cooperation as equilibria, and then we will consider how the two equilibria fare in the presence of noise in individual interactions.[22]

At first glance, punishment by members of the other group may seem most natural, since a defector in an interethnic interaction "offends against" the other

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or her (as in 3b), which gives them a positive incentive to carry out the punishment.

[20] Assume also that k and n are both even, and n is greater than 7.

[21] We briefly introduce in-group institutions that allow us to weaken this assumption below. An alternative approach, not taken here, would be to replace the assumption of full information with a mechanism of "gossip." For example, one may assume that if i's partner j defects, i reports this to all subsequent partners, they to their partners, and so on; those who have heard about j's defection then sanction j for a specified number of rounds. For any given δ, there will then be a size n^* such that for all group sizes $n \leq n^*$ cooperation can be supported by the threat of gossip "contagiously" spreading the news of a defection. Cf. Kandori (1992) and Ellison (1994), who consider how the fear of a contagious defection process that results in complete social breakdown rather than focused punishments (as in the case of gossip) may be used to support cooperation in the atomized information condition.

[22] Analysis of the third possible approach to sanctioning cross-group defectors adds little insight; it also proves less efficient off the equilibrium path than does either (1) or (2).

group. But this scheme faces a dilemma: If members of the other group cannot identify (or gain access to) the individual defector, then they cannot hold the individual personally responsible. It follows that to support cross-group cooperation solely by threat of punishment by the offended group, punishment must be indiscriminate, targeting either all members of the cheater's group or some random collection of them. While such random reprisals or feuding behavior may seem grossly unfair to innocent members of the defector's group, they follow, in a strategic sense, from the fact that it is harder to identify and check up on members of other ethnic groups than on members of one's own.

The following strategy profile describes a spiral equilibrium in which interethnic cooperation is based on the off-equilibrium path threat of indiscriminate, group-based reprisal for individual defections. In the event of a defection in an interethnic encounter, noncooperation rapidly spirals beyond the original parties to the dispute, involving coethnics in a collective effort to punish the other group.[23]

4. *The spiral equilibrium.* All individuals play the strategy σ_S, defined as follows. In in-group pairings, always play C with any player not in the in-group punishment phase (a "cooperator"), and always play D against any player in the in-group punishment phase (a "defector"), regardless of one's own status. A player enters (or restarts) the in-group punishment phase for T^{in} periods by defecting against a cooperator. In out-group pairings, play C provided that neither group is in the out-group punishment phase, in which case play D. A group enters that phase for T^{out} periods if any member defects in a cross-group pairing when neither group is in the out-group punishment phase.[24]

Thus, the spiral equilibrium rests on a sort of interethnic deterrence—individuals cooperate in interethnic interactions for fear of losing future payoffs should they defect and cause a larger breakdown of intergroup relations. Within groups, by contrast, cooperation is supported by individual punishment, and group members do not punish their coethnics for defecting against members of the other group.[25]

For σ_S to form an equilibrium, three key conditions must obtain. First, for individuals to want to cooperate in interethnic encounters, the immediate gains of defecting ($\alpha - 1$) must be less than the expected payoffs forgone due to the "spiral" breakdown (which are $\delta p + \delta^2 p + \ldots + \delta^{T^{out}} p$, since there is a p chance of being paired outgroup each period, and in this event one forgoes the benefits of cooperation [1] over defection [0]). This immediately implies that interethnic interactions cannot be too infrequent for a spiral equilibrium to be possible. If p is too small, the threat of a breakdown of intergroup relations is not compelling enough to induce individual members to cooperate.[26]

Second, cooperators cannot have an incentive to defect against other cooperators within their own group. Third, those who defect within the group must have an incentive to comply with their punishment (they must prefer to accept $-\beta$ for T^{in} periods to regain cooperation status over continual defection). In contrast to the first, these second and third conditions imply that interethnic interactions cannot be too frequent for intraethnic cooperation to be supportable; the more often individuals interact outside their own group, the less effective is the threat of in-group social sanctions.[27] It has been noted in the anthropological literature that ethnic groups are marked by institutions which maintain and police ethnic boundaries by sanctioning individuals who interact too much or in certain ways with outsiders (Banton 1983, Barth 1969, see also Laitin 1995a). In our model, an incentive to police ethnic boundaries and so delimit ethnic identity arises endogenously.

The second approach to giving individuals an incentive to cooperate in interethnic interactions is to have groups sanction their own members for cross-group violations of the norm. The following strategy profile characterizes an in-group policing equilibrium in which defections in interethnic interactions are completely ignored by the offended group, while the defector is sanctioned within his or her own ethnic group.

5. *The in-group policing equilibrium.* All individuals follow the strategy σ_{igp}, defined as follows. Play C in all out-group pairings. For in-group pairings, always play C with any partner not in punishment phase, and always play D against any player in punishment phase (regardless of one's own status). A player enters (or restarts) the punishment phase for T^{igp} periods by defecting when paired with an out-group member or a coethnic who is not in punishment phase.[28]

Under the in-group policing regime, the two groups in effect make a deal that benefits both sides. By adopting a policy of "you identify and punish your miscreants and we will do the same," they take advantage of the fact that each group has better information about the behavior of its own members than about the other group and so can target individuals rather than whole groups.

The conditions necessary for the in-group policing strategy σ_{igp} to form an equilibrium of the game are simpler and typically less restrictive than those for the spiral equilibrium. The reason is that a cooperator faces essentially the same incentives in both intra- and interethnic interactions. In both cases, defecting leads to T^{igp} periods of in-group punishment. Thus, equilibrium requires that cooperators have no incentive to defect in either in-group or out-group pairings (the same condition applies for both), and that defectors are willing to comply with their punishment in intraethnic interactions and have no desire to extend their punishment phase by defecting when paired with an ethnic other.

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[23] As a referee observed, this equilibrium does not represent a "spiral" in the sense of a dynamic process of action-reaction escalation. While punishment strategies that have this quality could easily be constructed, little insight would be gained in the present model. We intend "spiral" in the sense of rapid escalation beyond the immediate parties to the dispute.

[24] Necessary and sufficient conditions for σ_S to form a subgame perfect equilibrium of G (when it is chosen by all players) are given in proposition 2 in the Appendix.

[25] Note that we adopt the renegotiation-proof punishment strategy of (3b) for policing in-group cooperation here—the defector must accept being "the sucker" for T^{in} periods in order to be returned to the good graces of society. It is worth noting that while we also can adopt such a punishment regime for the interethnic interactions, the parameter conditions necessary to make this supportable as an equilibrium are extremely restrictive, because the infrequency of interethnic interactions greatly reduces the incentive to comply with being the sucker in the punishment phase.

[26] Formally, the necessary condition is that $p \geq (\alpha - 1)(1 - \delta)/\delta$.

[27] As shown in the Appendix, for these conditions to hold, the frequency of interethnic interaction p must be less than $1/(1 + \beta)$.

[28] Necessary and sufficient conditions for σ_{igp} to form a subgame perfect equilibrium of G are given in proposition 1 of the Appendix.

In contrast to the spiral equilibrium, under in-group policing the infrequency of interethnic interactions never poses an obstacle to intergroup cooperation—there is no lower bound on p below which interethnic cooperation is unsupportable.[29] The reason is that cooperation within the group has been linked to cooperation with out-group members. It does not matter whether one rarely interacts with members of the other group, since defecting in an out-group pairing will lead to sanctioning by in-group members with whom one interacts more frequently.[30]

Just as in the spiral equilibrium, however, maintaining intraethnic cooperation under in-group policing requires that interethnic interactions not be too frequent. Once again, the efficacy of in-group sanctions weakens as individuals interact more often with ethnic others, providing a rationale for institutions that police ethnic boundaries.[31]

Before considering how the spiral and in-group policing equilibria fare in the presence of "noise," we briefly examine a natural objection to the stability of in-group policing: Will groups be reluctant to sanction their own members for transgressions against the out-group? As we have specified σ_{igp}, there is no problem here, because σ_{igp} employs punishment strategies that actually benefit an individual who sanctions a defector; the strategy σ_{igp} requires that defectors make amends by cooperating while others in good standing defect against them. Thus, under σ_{igp}, individuals have a positive incentive to sanction people who have defected, no matter against whom.

Arguably, however, it is not always possible or realistic that sanctioning defectors will be costless to those who sanction. If it is costly—say, if players being punished and their partners both defect (as in equilibrium 3a), each getting zero rather than one—then the in-group policing regime is open to the above objection. Indeed, if we allow for explicit coordination and collusion within groups, then the in-group policing equilibrium may not be sustainable in this case. Consider the following proposal that could be made by a member of group A to coethnics when s/he is paired with a B: "I will defect and share the benefits $\alpha - 1$ with all of you if you agree not to punish me. Because of in-group policing, my defection will not lose us anything in interactions with the other group." If such in-group coordination is feasible, and for relatively small groups with their own internal governing apparatuses it probably would be, then the in-group policing equilibrium will break down in our model when sanctioning is costly.[32]

The problem in this instance is that, given our assumptions, groups have no way to verify that defectors in cross-group pairings are in fact being punished. In the real world, this seems an easy problem to solve—groups can simply make the punishment of defectors against the out-group public and observable to members of the other group, or they can just hand over the offender. Insofar as doing this is costly or difficult for the group harboring the offender, it may be necessary for the other group to provide an incentive in the form of a threat to return to a spiral equilibrium. In other words, failure to publicly sanction one's coethnics for violations against the out-group will lead the out-group to begin a spiral or feud. Thus, one can imagine an in-group policing regime that rests or depends on a deeper threat to resort to widespread feuding (or total avoidance); the threat of feuding induces in-group policing.[33]

Efficiency in the Presence of Accidents and "Noise"

The spiral and in-group policing regimes suggest how ethnic groups may manage to maintain mutually beneficial cooperative relations as the normal state of affairs, despite the obstacles posed by asymmetric information and the relative infrequency of interethnic interactions. And if play is kept to the equilibrium path, the spiral and in-group policing regimes are equally good in this respect.[34] But occasional defections (or perceived defections) are inevitable, due to mistakes, misinterpretations, drunkenness, sudden passions, or unobservable variations in payoffs from encounter to encounter. We need to ask, then, how the two regimes perform in the presence of such "noise."

Intuitively, one might expect fallible individuals to fare better under in-group policing than under the spiral regime. In-group policing focuses punishment on individuals, whereas in a spiral regime the whole group suffers a "collective bad" from the transgression of one member. Furthermore, if the groups are moderately large and there are many interethnic interactions in each period, then the probability that noise will produce a defection in at least one pairing is high, even if the amount of noise is low. This can mean that the groups spend most of the time not cooperating, or that the

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[29] See the Appendix for a statement of parameter conditions under which both, neither, or only one of σ_{igp} and σ_S are supportable as equilibria. Briefly, if $\alpha < 1 + \beta$ (which is likely when defection is understood to entail violence), the in-group punishment regime is less restrictive in the sense that, for all other parameter values, if σ_S forms an equilibrium, then so does σ_{igp}, while the converse is not true.

[30] In an entirely different context, Lohmann (1997) shows how linked punishment strategies can support cooperation for lower δ's than would be possible without linked strategies.

[31] The upper bound on p is the smaller of $1/(1 + \beta)$ and $1 - ((\alpha - 1)/(1 + \beta))$.

[32] This problem can arise even with renegotiation-proof in-group punishments like those of σ_{igp}, if the benefits of cheating an ethnic other exceed the benefits of sanctioning a defector.

[33] There is quite a bit of evidence for this in the anthropological literature. See Colson 1974, chapter 3; Miller 1990; Moore 1978.

[34] They also will be observationally equivalent, without soliciting information about peoples' beliefs.

TABLE 2. Performance of the Two Equilibria with "Noise" in Interethnic Interactions: Average Per-period Expected Equilibrium Payoffs for $\alpha = 1.1$, $\beta = 2$, $\delta = .95$

p	k	ε	V^*_{igp}	V^*_S	T^{out}_{min}
.05	100	.1	.973406	—	
	(n = 2,000)	.01	.997251	—	
		.001	.999724	.976064	6
		.0001	.999972	.997426	3
		0	1	1	3
	1,000	.1	.973407	—	
	(n = 20,000)	.01	.997251	—	
		.001	.999724	—	
		.0001	.999972	.976137	6
		0	1	1	3
.10	100	.1	.948266	—	
	(n = 1,000)	.01	.994641	—	
		.001	.999462	.974636	2
		.0001	.999946	.996420	2
		0	1	1	2
	1,000	.1	.948267	—	
	(n = 10,000)	.01	.994641	—	
		.001	.999462	—	
		.0001	.999946	.974839	2
		0	1	1	2
.20	100	.1	.902293	—	
	(n = 500)	.01	.989836	—	
		.001	.998980	.970113	1
		.0001	.999898	.996250	1
		0	1	1	1
	1,000	.1	.902297	—	
	(n = 5,000)	.01	.989836	—	
		.001	.998980	—	
		.0001	.999898	.970568	1
		0	1	1	1

Notes: V^*_{igp} and V^*_S give time-averaged per-period equilibrium payoffs for the in-group policing and spiral equilibria, respectively. A dash means that σ_S does not form a Nash equilibrium for these parameter values. T^{out}_{min} is the smallest T^{out} such that σ_S is supportable, given ε.

spiral regime breaks down entirely because the anticipation of noise producing a spiral gives individuals an incentive to defect *deliberately* in interethnic encounters.

These intuitions are borne out when we introduce a small amount of noise into the model. For simplicity, we assume that mistakes and misinterpretations sometimes occur in the interethnic pairings, while there is no such noise in intraethnic interactions.[35] Assume that in every interethnic pairing there is an $\varepsilon > 0$ chance that a player who intends to cooperate accidentally defects. For several different sets of parameter values, Table 2 gives an individual's ex ante expected per-period payoff given such noise under both the spiral and in-group policing regimes.[36]

Two main points emerge from these examples. First, in-group policing yields higher expected payoffs for all members of both groups than does the spiral equilibrium.[37] The size of the improvement varies in these examples from 0.5% to 2.5% per period, and improvement up to 10% per period can be generated in examples with a larger α.

Second, the spiral equilibrium is much less robust against the introduction of small amounts of noise in interethnic interactions than is in-group policing. Notice that the spiral regime typically ceases to be an equilibrium if ε exceeds even a tiny threshold, while the payoffs under in-group policing are scarcely affected by variation in ε. In these examples, ε has to be on the order of $1/10pn$ for players to have an incentive to cooperate with ethnic others under the spiral regime (recall n is group size, and p is the interethnic interaction rate). The reason for the fragility of the spiral regime is obvious. The odds that an accidental defection will occur in at least one interethnic encounter increase rapidly with the number of encounters. Thus, even for relatively small communities (e.g., 1,000 interethnic interactions each period), a small amount of noise (e.g., $\varepsilon = .001$) will still be associated with a relatively high risk of breakdown each period (in this case, 86% each period). Thus, there is a large temptation to defect on purpose since a breakdown is likely anyway.

One might try to render the spiral regime more robust by raising the threshold that triggers a spiral. Consider, for example, an equilibrium in which defection must occur in at least $j > 1$ interethnic encounters in a period for a breakdown to follow. Then, for an appropriately chosen threshold j, individuals might be deterred from defecting against ethnic others by the risk that they will tip relations over the edge. Perhaps we could then maintain the spiral regime by raising the threshold as group size increases.[38]

Though we lack the space to give the details, the scope for improvement here turns out to be almost nil. Briefly, when the number of interethnic interactions is moderately large, the *marginal* probability that any one person's defection will tip the groups into conflict is infinitesimal for all but the lowest thresholds (i.e., one or two), so we are back to the problem that a small amount of noise can imply a large risk of breakdown. In essence, with groups of moderate or large size, players face a collective action problem under the spiral regime. Opportunistic behavior with ethnic others is individually beneficial but socially costly due to indiscriminate punishment. In-group policing may be seen as an arrangement which, if effective, dramatically lessens this collective action problem. Indeed, a spiral regime may have a natural tendency to develop features of in-group policing, as it makes sense for individuals to try to prevent others in their group from provoking conflict that hurts all.[39]

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[35] This conforms with the notion that cultural (and perhaps language) differences make misinterpretation relatively more likely in interethnic interactions.

[36] The table gives time-averaged payoffs, which are the players' ex ante expected value for the game multiplied by $1 - \delta$. See the Appendix for the expressions that yield these numbers. Note also that the expected per-period payoff would be 1 without noise, and without any interethnic cooperation it would be $1 - p$.

[37] We are unable to find any parameter values such that the reverse holds for σ_{igp} and σ_S. If in-group punishment strategies are changed to make sanctioning costly, then for very small groups it can be possible for the spiral equilibrium to be more efficient with small amounts of noise.

[38] The origins of this idea are in Green and Porter (1984), who showed how oligopolists may police collusion despite imperfect monitoring by appropriately setting a "trigger price" below which the firms begin a price war. For applications in political science, see Downs and Rocke 1990, 1995.

SOME ALTERNATIVE ASSUMPTIONS AND ADDITIONAL IMPLICATIONS

In this section we consider the effects of weakening and varying some of the assumptions that underlie the game studied above. New insights emerge, and the core insight proves robust, namely, that two quite different types of social institutions can enable interethnic cooperation in domains in which the state is weak or absent.

Interethnic Interactions that Are Not Anonymous

For the game G, we assumed that individuals cannot subsequently recognize or reliably identify ethnic others who cheat or exploit them in interethnic encounters (or that they cannot reliably communicate this information to their ethnic brethren). Suppose instead that interethnic encounters are not anonymous in this sense, so that if a B cheats an A, the other members of the A group observe the identity of the B player (or can learn it reliably). In that case, interethnic cooperation may be supported by focused and individual rather than indiscriminate punishment; i may be deterred from cheating an ethnic other j by the prospect that i (and i alone) will be "boycotted" or otherwise sanctioned by the members of j's group. Thus, no spiral of defection beyond i and j need occur. There are two problems with this regime, however.

First, insofar as interethnic interactions are still relatively infrequent, the force of the threat by j's group to punish i may be undermined, just as in the spiral regime case. Even if members of j's group recognize i, they may rarely encounter or have great difficulty finding i. An in-group policing scheme thus again appears as the natural alternative solution (short of more complex state-based solutions). Second, if all players on both sides do not observe the interethnic interaction in which i cheats j, then the potential for a new kind of spiral or feud emerges. Namely, if members of j's group sanction i, then members of i's group may be unsure (or misled) as to whether this is retaliatory punishment or a transgression that in turn requires sanctioning. Thus, imperfect observability of interethnic encounters may imply that even focused cross-group punishment schemes will be accompanied by periodic tit-for-tat spirals and feuds.

Intraethnic Institutions for Maintaining In-Group Cooperation

For G, we assumed that individuals observe (or learn about) all intraethnic interactions each period. This strong assumption was intended as a tractable stand-in for more plausible institutional mechanisms that groups may use to support cooperation, such as gossip and quasi-formal institutions like Milgrom, North, and Weingast's (1990) law merchant (see also Calvert 1995). Here we briefly consider the implications of introducing such institutions.

Assume atomized information, so that individuals observe what happens only in their own interactions. Add an additional player to each group, called the mediator. The mediator engages in no transactions, but every other individual in the group has the opportunity to appeal to the mediator after each interaction at a small personal cost, $c > 0$. For in-group interactions, if i appeals to the mediator of her group in period t, the mediator ascertains what happened in the interaction between i and j and awards a judgment of size $\gamma > 0$ (in utility) to the aggrieved party if one of the two cooperated while the other defected.[40] If the award is paid, then the mediator does nothing further; if the award is not paid, then the mediator communicates the name of the guilty party (say, j) to all other members of the group, and j is then socially sanctioned for T^{igp} periods. (The mediator may announce j's status in a public forum or spread the word via other notables.) Cooperation within the group will then be supportable, despite atomized information, if there is an award level γ large enough to deter defections but small enough such that it is worthwhile for defectors to pay the compensation, given δ and the punishment regime.[41]

What effect would such in-group mediators have on interethnic cooperation? On the plausible assumption that each group's mediator lacks detailed knowledge of and social connections in the other group, the same set of two cooperative options, spiral and in-group policing, will emerge. Suppose an A player, i, is cheated or exploited by a B player, j. Appealing to the A group's mediator is of no use if s/he cannot identify or locate j, or if s/he lacks the connections and authority to disseminate a negative ruling among group B. So, to support interethnic cooperation, either (1) the two groups coordinate on an in-group policing arrangement, wherein i appeals directly to the mediator of group B for compensation or goes to the A mediator, who then brings the case to the B mediator, or (2), failing this, the groups threaten a breakdown of relations or feud due to interethnic transgressions, to be announced and coordinated within A and B by the in-group mediators (that is, a spiral regime).

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[39] As Miller (1990, 198) argues with respect to Icelandic bloodfeuds, group liability "had the effect of inducing people who might be held accountable for each other's actions to involve themselves in each other's affairs" and thus made the feud "much more effective as an instrument of social control than it would otherwise have been if only the actual wrongdoer suffered the consequences of his actions." On this point see also Colson 1974, 40–1; Gellner 1988; Hardin 1995, 118–9; and Moore 1978.

[40] The mediator makes no award if either CC or DD was played. The assumption that the mediator can discover what happened in the interaction (which is also made by Milgrom, North, and Weingast 1990) can be weakened so that the mediator may err.

[41] This brief statement does not address the issues of paying the mediator, which can be arranged in a variety of plausible ways; ensuring that mediators diffuse information about violations, which may require implicit threats to cease using the mediator if she does not do this; and ensuring that the mediator does not turn to extortion, which may be addressed by a competition among potential mediators, such that they have an incentive to cultivate a reputation for honesty or else lose respect and business (cf. Milgrom, North, and Weingast 1990; Calvert 1995). Note that this intraethnic institution is only one short step from a state-like policing apparatus; the only difference is that enforcement remains decentralized.

Distinguishing Types

The game G modeled interactions in which all individuals have an incentive opportunistically to exploit their partners. For many interactions, however, such as routine encounters in the street, it is more realistic to suppose that people vary in their disposition to behave opportunistically (or aggressively). Furthermore, it is reasonable to suppose that people are better at assessing the likely disposition of coethnics they have never met than of ethnic others they have never met, due to a lifetime of training in the cultural habits and signs of their own ethnic group. The strategic consequences of this differential ability to sort among types can be usefully explored in a variant on the game G.

Suppose there are two types in both groups—"good" types, who gain $\alpha_g < 1$ for defecting when their partner cooperates, and "bad" or opportunistic types, whose value for defecting on a cooperator is $\alpha_b > 1$. Otherwise, both types' payoffs are as in Figure 1. Assume further that in in-group interactions players observe the type of their partner in each period prior to choosing strategies, while in interethnic interactions they do not observe their partner's type[42]; that the proportion of bad types is the same in both groups, a small fraction $q > 0$; and that individuals observe only what happens in their own interactions (atomized information).

Supporting cooperation among the good types within ethnic groups is then a simple matter, even under atomized information. The good types cooperate if matched with a good type and defect if matched with a bad type, yielding them an average in-group payoff of $1 - q$. (Bad types defect against all partners.) The problem arises with interethnic interactions. If the proportion of bad types is believed to be high enough (if $q > (1 - \alpha_g)/(1 - \alpha_g + \beta)$), then good types will prefer to defect in interethnic interactions to protect themselves against the risk that they are facing a bad type, making the payoff for all interethnic interactions zero in equilibrium. Thus, we have a very different argument for why cooperation may be more difficult to sustain in inter- than in intraethnic interactions, one based not on asymmetric information resulting from different social networks, but on differential abilities to distinguish types inside versus outside the ethnic group.[43]

There appear to be two basic institutional approaches the groups may take to overcome this problem, short of constructing an effective, overarching police apparatus. To gain interethnic trust and cooperation, opportunists need to be induced to cooperate despite their inclination, which may be achieved—once again—either by threatening indiscriminate retaliation (the spiral regime) or by arranging for the in-group sanctioning of opportunists.

Groups of Unequal Size

If one group (say, A) is larger than the other, then the rate of interethnic interaction decreases for the larger group and increases for the smaller group. For example, if A has 10,000 members, B has 1,000, and there are 200 interethnic encounters per period, then the probability for an A of an interethnic encounter is $p_A = .02$, while it is $p_B = .20$ for a B (under uniform matching).

This implies that the threat of spiral punishment cannot be used effectively by small groups against large ones, since As who defect against Bs interact so rarely with Bs that the Bs threat of a breakdown of relations is not compelling (as in the case of equal-sized groups with small p, discussed above). By contrast, the threat of spiral punishment is highly credible and effective as an instrument by which large groups may induce cooperation by small groups, since a breakdown of relations is very costly for members of the small group who interact often with the members of the larger group. Thus, we would predict that small groups will be more likely to evolve in-group policing strategies to try to avoid the costs of group punishment, while the threat of indiscriminate punishments will be more typical of how large groups give members of small groups an incentive to cooperate. In addition, note that the efficiency gains of setting up an in-group policing regime are small for members of the large group since their rate of interethnic interaction is small. Thus, it may be difficult to provide incentives for the members of the larger group to cooperate with the smaller group, since the costs of setting up in-group policing may increase with group size, and members of the larger group lose little from conflict with the smaller group.

In addition, if the members of small groups tend to have relatively high interethnic interaction rates, then such groups may need to develop strong boundary policing institutions in order to maintain themselves as distinct groups. As seen in the previous section, high interethnic interaction rates tend to undermine a group's ability to maintain cooperation and harmony within the group by reducing the force of in-group sanctions. Small groups, our model suggests, will seek either to assimilate into the larger group's social control mechanisms or to develop strong boundary policing institutions and practices, some of which may intentionally limit the amount of interethnic interaction in which their members may engage.[44]

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[42] We could add a (more realistic) stochastic element to peoples' observation of type, but this gains little insight.

[43] Cornell (1995) suggests essentially this argument informally. Note that this mechanism may help explain why individuals of the same ethnic group may be more inclined to trust one another even if the group is very large and they have no common network connections.

[44] Of course, the larger group may also attempt to police its boundaries by resisting assimilation, either for the reason identified in the model or (perhaps more likely) to protect low-status members of the group who are threatened by competition from the minority. See Laitin 1995a for an analysis of boundary policing and marginal groups.

EMPIRICAL ILLUSTRATIONS AND ASSESSMENT

The model and theoretical analysis presented above readily explain two important empirical aspects of interethnic relations that appear in a very broad range of cultural contexts. First, many have observed that interactions between individuals from different ethnic groups are marked by a distinct tension and lack of trust, which we explain as a strategic consequence of problems of asymmetric information due to the lower density of social networks across groups and to the differential ability to distinguish types inside versus outside the group. Second, as we argued earlier, despite greater tensions, peaceful interethnic relations are by far more common than violence or complete avoidance, which we explain as a result of institutional mechanisms that moderate cross-group problems of opportunism and so allow people to avoid the costs of violence and capture the benefits of peace. Existing rationalist and psychological theories of ethnic conflict, which argue that violence results from shifts in relative power, status, or access to resources, cannot so easily explain these facts. For example, it is not clear on either account why relations among *individuals* from different ethnic groups would be marked by any unusual tension or lack of trust. Furthermore, these accounts cannot help but be somewhat ad hoc in explaining how interethnic relations can be simultaneously tense and peaceful almost all the time.

Nonetheless, while our theory is consistent with these empirical regularities, there surely are plausible alternative factors involved as well. For instance, simple prejudice may account for much of the tension observed in interethnic interactions, and the activities of (centralized) state institutions may be crucial for explaining why interethnic peace is the modal outcome in many places. Our claim is not that the mechanisms we have identified are the only ones that matter, but that they have not been clearly identified before and do explain a part of the empirical puzzle.

If the latter part of this claim is correct, then we should be able to find empirical examples in which the causal mechanisms we have identified appear and matter as predicted. Specifically, we should be able to find evidence that asymmetric information creates problems of opportunism in interethnic interactions and that institutional mechanisms mimicking or paralleling in-group policing and spiral equilibria have evolved to deal with them. We consider a range of such examples in this section. While the examples support our core theoretical claims, at the same time they suggest that the diversity of institutional responses to the problem we identify is greater than that captured by our model.

Asymmetric Information and Tension

A number of observers of interethnic relations have noticed that the lack of information about ethnic others as individuals is connected to interethnic tension. Boehm (1994), in his ethno-history of Montenegrin feuds, distinguishes between Christian-Muslim disputes, which were nearly impossible to control, and feuds among Christian clans. He reports that headhunting, the most dreadful insult possible in Montenegrin culture, was normal in wartime. It was most likely done "against Moslems who lived at a considerable distance, where the individuals would not know exactly who had killed whom in the engagement" (p. 91). This tactic was not ordinarily used in blood feuds, since families knew each other. Not knowing the other as an individual, Boehm implies, loosens the constraints on "defection" and also allows for more indiscriminate retaliation. Similarly, in her study of Albanian blood feuds, Hasluck (1954, 226) notes: "If an Albanian was killed by a Slav, any Albanian would kill any Slav in revenge," whereas within Albanian settlements, more focused retaliation was both possible and enjoined by norms.

Particularly insightful on this issue is Rieder's (1985) study of the Jewish-Italian alliance against African Americans in Canarsie. When Jews and Italians could not recognize African Americans as individuals, or even as members of a social class, it was very difficult to develop cooperative relations with them.

> The ambiguous position of the new blacks deprived white residents of adequate strategies for promoting their own well-being, deciphering the environment, and appraising risk. More than a tiresome slight, the notion that one could not tell blacks apart bespeaks the intricate way urban dwellers scan their environment, sift visual and other clues for forecasts of danger, and make guesses about the intentions of strangers.... However imperfect or superstitious their basis, predictive clues reduce the feeling of risk, and they are often more than consoling fictions. The signs that elicited alarm included race, but not indiscriminately. Young black males, especially in groups, coiffed with Afros or sporting sneakers ("felony footware") triggered apprehension (Rieder 1985, 86, 177).

In this example as in several others given below, the ethnographer links the specific tensions that mark interethnic relations to an inability to distinguish among individuals and types of people in the other group.

The Evolution of Institutional Mechanisms

When it is not possible to identify the guilty party as an individual, one feasible strategy for policing cooperation is collective liability, or group punishment. All members of the other ethnic group are held equally responsible for the actions of any one of them. If a member of one group is murdered, for example, the other group seeks retribution by killing some random person of the other group. To a considerable extent, this is the way the Israeli Defense Forces handled Palestinian raids inside the Green Line for many years, and it also is the way that Palestinian nationalists targeted Israelis after what were perceived as land grabs. It was not necessary for either party to identify the person on the other side who was guilty of the transgression, only to strike back at some set of members of the other group with equal or greater damage. While this mechanism addresses the problem of low information, it fosters spirals of violence, as we discussed more formally above, and it also may systematically enrage a wider circle of innocent victims. Indeed, the solution of Israeli Labor Party leaders, which continues as the declared policy of Prime Minister Netanyahu, looks very much like in-group policing[45]—the Israeli government grants some autonomy to the Palestinian Authority on the condition that Arafat develop effective in-group policing.

Since indiscriminate retribution is not efficient in the presence of noise, we may find evidence of the evolution of other, more efficient institutional arrangements. One problem with searching for such mechanisms is that anthropologists are in the best position to study localized conflict management, but few of them have focused on inter- as opposed to intraethnic conflict resolution. Thus, our examples mainly were found at the margins rather than at the center of anthropological studies. Another problem is that many of the examples of in-group policing developed under imperial authority, so we cannot rule out the possibility that the inducements of a central state are necessary for their smooth operation. Nonetheless, these examples demonstrate that in-group policing plays an important role in cauterizing conflict when members of different groups have low information about one another as individuals.

In some cases, in-group policing is provided not by formal arrangement between groups but as a private strategy of individuals to facilitate trade across ethnic boundaries. Craig R. Whitney reported an excellent example in the *New York Times* (June 16, 1995). In Cocinelle, France, Miro Rizvic, manager of a grocery store, adjusted as follows to the problem of low information under conditions of ethnic heterogeneity that plagued other store owners: "Most of the crimes are committed by young North African kids.... [So] I hire local people, a Moroccan for the vegetables, a Tunisian for the canned goods, a Turk for the cash register. That way they know just about everybody who comes in here. It's better than hiring security guards."[46] (Note also how this example supports the claim that information asymmetries create problems of opportunism in interethnic relations.)

Clifford Geertz (1973, 7–9) gives a related example of a merchant who made private arrangements to overcome his inability to recognize individuals within a different ethnic group. In this case, the merchant contracted with agents specializing in information and purchased insurance from them in order to identify predators. Geertz relates that in early colonial Morocco, a marauding band of Berbers attacked the home of a Jewish trader in the Maghrib named Cohen. He survived but his guests were killed and his goods stolen. Cohen could get no help from the French authorities, but he belonged to a *mezrag*, or trade-pact system, and he went to his insurance broker, a tribal sheikh, to demand the assistance due. The sheikh knew precisely who had Cohen's merchandise, accompanied him in a climb up the Atlas directly to the shepherd of the thief's tribe, and took control of the entire herd. The tribal warriors soon returned, saw what had transpired, and prepared to attack. But then they saw Cohen and his insurance agent, a palaver began, and Cohen peacefully regained his goods at the precise insured value. Note that "on the equilibrium path" this institutional innovation of tribal "information brokers" would make mutually beneficial trade relationships between Jews and Berbers possible, despite problems of opportunism due to a low density of social network relations. And, in the case Geertz relates, the institution also prevented spiraling, here understood as a total breakdown of trading and relations between Jews and Berbers.

Somewhat more institutionalized is the arrangement that operated until recently among Armenians and Azeris living in (what were) mixed regions in Armenia and Azerbaijan. There was a formal relationship of "friendship" (*kirva* in Armenian), often initiated by inviting the potential "friend" from the other group to a child's circumcision ceremony; an Armenian in one village and an Azeri in another agreed to aid the other in any business or other dealings with members of their own ethnic group. This help might involve settling disputes, providing information about people with whom to trade, or facilitating connections in the "foreign" community.[47] Presumably, an outsider with such a friend was less likely to be cheated because that friend might be able to harm a defector's reputation within the group.

Examples of institutionalized in-group policing are abundant under conditions of imperial rule. In the millet system in the Ottoman Empire, each religious/nationality group (the core groups were Orthodox Christians, Armenians, and Jews) received a high degree of autonomy as long as their leaders successfully policed their own communities within their millet and monitored members of their own community in interactions with outsiders. Consider anti-Semitic incidents in Turkey in the second half of the 19th century. According to Paul Dumont, there was intense antagonism. Despite intermingling of Jews, Muslims, and Christians in "apparent harmony," Dumont states (1982, 222–3),

> the slightest spark sufficed to ignite the fuse. Whenever a young Christian disappeared at the approach of Passover, Jews were immediately accused of having kidnapped him to obtain blood necessary for the manufacture of unleavened bread. Threats and violence followed close behind the suspicions and generally things ended with a boycott of Jewish shops and peddlers.

This is an example of indiscriminate group punishment, and Dumont recounts many instances in which all Jews suffered for any supposed transgression against Christians. A more nuanced picture appears, however, in his discussion of a riot in 1874 in Urla, involving the disappearance of a young Greek girl. Although she "eventually reappeared, Jews were persecuted and boycotted for several weeks. Here the governor of the province was obliged to order Orthodox priests to preach peace and goodwill in their churches" (pp. 221–2). The message from the Ottoman authorities was clear: Monitor the defections of your own group or you will be replaced by someone else as millet leader. Thus, the normal functioning of the system, according to which the leaders of each millet curbed excesses in violence against members of other millets, helped ensure that "while in Russia, Rumania, and most of the Balkan states, Jewish communities suffered from constant persecution (pogroms, anti-Jewish laws, and other vexations), Jews established on Turkish territory enjoyed an altogether remarkable atmosphere of tolerance and justice" (pp. 221–2).

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[45] See, for example, N. R. Kleinfield, "Netanyahu Promotes Views on Peace and Investments," *New York Times*, 12 July 1996, sect. A.

[46] Whitney adds: "Mr. Rizvic said that he was now a French citizen, but that he, too, emigrated 20 year ago—from Bosnia."

[47] This information comes from personal communications with Sergei Arutiunov.

In much of premodern Europe, state authorities relied heavily on in-group policing in Jewish affairs, institutionalized in kahals, or self-governing communities (Dubnow 1916, 105–6; Goldscheider and Zuckerman 1984, 18–24). Based upon a 1551 charter of Sigismund Augustus, for example, Jews in Poland were given the right to elect their own rabbis and "lawful judges." In these kahals, Jews received local autonomy in education, religious affairs, and internal conflict management. In return, the kahal leaders took full responsibility for the collection of government taxes. We have found fragmentary evidence that the kahals engaged in forms of in-group policing that went beyond identifying tax evaders. In 1672, to cite one example, the Polish king "bestowed upon the Kahal elders of Lithuania the right of excluding from the community or of punishing by other measures those recalcitrant members of their Kahals who by their acts were likely to arouse the resentment of the Christian population against the Jews" (Dubnow 1916, 190). This suggests that kahal leaders identified Jews whose acts hurt non-Jews—one example being the passing into general circulation of clipped or counterfeit coinage (Lederhendler 1989, 29)—and punished them.

After the abolition of the kahal in Russia in the mid-19th century, the czarist regime began to attach Russian-speaking Jewish advisers to various offices of the bureaucracy, to "impose greater order and direct supervision on the Jewish population" (Lederhendler 1989, 92), and they presumably played a key role in identifying lawbreakers to state authorities. One quasi-institutionalized role that transcended the kahal and post-kahal periods is that of *shtadlan*, a Hebrew term designating "public-spirited Jews who defend the interest of their coreligionists before the Government" (Dubnow 1916, 111). These *shtadlanim* regularly informed on members of their own community to state officials. In fact, Lederhendler emphasizes that "informing" on the treasonous behavior of fellow Jews was common practice from the kahal period onward (1989, 12–4, 28–9, 88–92). Reciprocally, state authorities policed the non-Jewish communities to prevent attacks on Jews, which largely contained pogroms for more than three centuries, despite persistent local anti-Semitism. It was not until 1881 and the chaos following the assasination of Czar Alexander II that hostilities against Jews went unchecked. In-group policing on both sides (though the Russian and Polish states were not motivated by a desire to prevent indiscriminate reprisals by Jews so much as by a desire to maintain the tax base) may help explain the long period of relative peace.

Another example of in-group policing comes from Yorubaland under British rule. When Hausa traders began dominating the kola and cattle trade in Ibadan, many interethnic conflicts arose that were quite difficult to manage and occasionally spiraled. The solution for the Yoruba government (under a watchful British imperial eye) was Hausa autonomy within carefully circumscribed neighborhoods (called *sabos*). As Cohen (1969, 21) describes the process,

> a number of Hausa commission agents would lodge temporarily with Yoruba house-owners, and then, as their number increased, and as they were joined by men from related occupations, they constituted a social problem within the polity of the settlement. The Yoruba population would complain that the Hausa were harbouring burglars, pickpockets and other categories of "undesirables" who endangered law and order in the community. To meet this danger, and also to meet the simultaneous clamour of the Hausa to live on their own . . . the local chiefs would decide to allot to the Hausa a special Quarter within the settlement and would recognize one of the Hausa men of influence . . . as chief. He would then be held responsible to them for the conduct of his people, inform the authorities about the undesirables in his community, and help in the collection of taxes.

It may be asked why Hausa chiefs in the sabos (or Jews in the millet and in the pale) would agree to inform higher authorities about the identity of their "defectors." Our theoretical analysis suggests an answer. Smaller ethnic groups within a larger society have a great percentage of interactions with outsiders—nearly all Hausa transactions in the kola and cattle trades were with Yorubas—and therefore suffer greatly from a breakdown of relations. Thus, they face strong incentives to set up effective in-group policing institutions.

As our theoretical analysis suggested, in-group policing may be accomplished in many instances through professional mediators who know one or both groups well and who specialize in extracting precise information from disputing parties to design finely calibrated compensation packages that prevent spiraling violence. Parallel to what Milgrom, North, and Weingast (1990) call law merchants, we find evidence that "mediation merchants" with a reputation for objectivity in interethnic disputes are hired by feuding parties.

An example is given by Samatar (1982, 36–54), who describes a war between a nomadic and a farming clan in eastern Ethiopia, while the state was quite content to see the clans decimating each other. The source of the problem was famine, which brought the nomads into areas traditionally rangelands but recently become farmland. One evening the herds of the nomads wandered into a maize field and damaged the crop. A skirmish followed, in which a nomad and a farmer were killed, and for three months young toughs of both clans roamed the regional city with rifles, shooting indiscriminately at members of the other group. This spiraling and indiscriminate punishment reached a climax with the murder of a nomadic elder. The nomads began to recruit an army from their wider kin, seeking to teach this "worthless scum of saucy slaves" (the nomads saw themselves as nobles and the farmers as former slaves) a lesson.

On the night the nomad force planned to attack, a group of nomad elders who had trade relations with the farmers stood in front of the attackers and demanded a council to determine whether there should be war. The leader of the warriors gave a powerful speech, calling upon Islam and wounded pride and demanding the destruction of the farming community. But the leader of the peace group used a plea of common Islam and then appealed to a common Somali interest (shared by the farmers and nomads) against the Ethiopians, who were confiscating the farmers' cattle and the nomads' sheep and camels. This leader called for a delegation to work out a proper compensation scheme, identifying precisely who required redress. The peace platform won, and the conflict was, through negotiation and compensation, toned down. The settlement along the lines of in-group punishment—targeted compensation packages and the aid of coethnics in identifying who is liable within the group—was made possible by a mediation merchant, who was Samatar's father.

One way mediation merchants can matter, as suggested by Morrow (1992) but not by our model, is that they can present a solution which is agreeable to both sides but which the disputants may be unwilling to propose for fear of appearing "weak." In Montenegrin conflicts, Boehm (1994, 122) notes that "Courts of Good Men" were mediators whose members had sufficient prestige to settle feuds. They could be asked to intervene only after a third successful truce had been called. Customarily, the clan with the higher score (i.e., had caused more bloodshed than it suffered) initiated a truce, but this was seen as a sign of weakness, and a great deal of outside pressure or war-weariness usually was necessary before a first truce was achieved. In other words, mediators can propose solutions that participants themselves want but are reluctant to raise themselves, and for this very reason the combatants may be unwilling to propose calling upon mediators. Nonetheless, to the extent that mediation merchants are available for hire, heated interethnic tensions are more likely to cool. As in the case of insurance schemes, mediation merchants can fill in the information gaps in ways that address the problem identified in the in-group policing equilibrium.

We stipulated in our model that one reason interethnic conflict has a high propensity to spiral is that members of one group have low information about past behavior of members of out-groups. By lumping all outsiders together, it is more difficult in prisoner's dilemma matching games to restore cooperation when it breaks down. In this section we have shown that the problem has real-world analogues. We have also shown that various institutional mechanisms have evolved to ameliorate the potential consequences of this problem, mechanisms that mimic or parallel those found in our model.

CONCLUSION

To conclude, we will summarize how we have addressed the three explanatory puzzles that began the paper, and we then consider several ways that our model may be profitably extended.

Why are interethnic relations frequently characterized by a tension that is relatively absent in intraethnic relations, giving interethnic relations, even when peaceful, an ominous quality? Because social networks are better developed and interactions more frequent within ethnic groups, individuals have easier access to information about their coethnics than they do about ethnic others. Better information and more frequent interactions allow coethnics to develop and maintain *individual* reputations for cooperative behavior that are more difficult to sustain in interethnic interactions. In addition, due to cultural familiarity, people are better able to distinguish opportunists among coethnics, which facilitates peaceful interaction within and hampers it across ethnic groups.

Why in interethnic relations is cooperation the more likely outcome than escalating violence, despite the greater tensions in day-to-day affairs? Due to the costs of persistent violence and the various benefits of peaceful interethnic relations, decentralized institutional arrangements are likely to arise to moderate problems of interethnic opportunism.

Why in some cases do interethnic relations often remain cooperative for a long time yet periodically break down in the form of spiraling violence, while in other cases peace is quickly restored after violence breaks out? We have identified two basic institutional approaches to resolving problems of opportunism in interethnic interactions. In both, interethnic cooperation is sustained by the expectations people have about what will happen if they cheat or otherwise injure someone from another group. The content of these expectations differs markedly, however.[48] In one type of equilibrium, people expect that if cooperation breaks down due to an accident, mistake, or misinterpretation, then members of each group will punish each other indiscriminately for some length of time (long enough to deter defections in typical interactions). These expectations are self-fulfilling. We call this a spiral equilibrium because violence or noncooperation quickly expands beyond the initial parties to a dispute. It is indistinguishable from the other cooperative equilibrium when play remains on the equilibrium path, but it can be quite unpleasant off the equilibrium path.

We also identified an institutional arrangement that is more efficient than the spiral equilibrium in the presence of noise. Under in-group policing, people expect that someone who defects against an ethnic other will be identified and sanctioned by members of his or her own group. Again, these expectations are self-fulfilling, although in practice institutional arrangements that make the punishment visible to the offended group may be necessary.[49] Examples of the evolution of institutions that resemble in-group policing mechanisms demonstrate the feasibility of this sort of institutional arrangement.

How can the model presented here be developed and extended? We have focused upon decentralized mechanisms that delimit interethnic violence and have claimed that these can work in the absence of state authority.

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[48] Greif (1994) argues more generally that "culture" may be productively understood as shared beliefs about what happens off the equilibrium path.

[49] The establishment of a formal office to see to this—such as the *shtadlan* and the chief of the Hausa quarter—may be interpreted in these terms.

Nonetheless, the central state appears in the background in some of the empirical examples we gave, and its role may be significant. In fact, in our vignettes of interethnic relations in three empires (the Ottoman, Russian, and British), systems of in-group policing were certainly related to indirect rule, which also may have made them possible. Further development of our informational approach would require fuller consideration of the state's role in both cauterizing and fostering interethnic violence.

One approach in the context of the present model is to think of the state as a player who will intervene only if violence reaches a certain level. African Americans in Cabrini Green in Chicago know that gang wars within their housing project can escalate up to some specific level before the city authorities intervene. Of course, if a gang were to terrorize a nearby wealthy neighborhood in Lincoln Park, the threshold would be lower. In the race riots in Los Angeles after the Rodney King verdict, police quickly moved into areas in which the state often *seems* absent. The threat of state intervention was explicit in the case of the Jewish near-pogrom in the Ottoman empire. Members of conflicting groups, then, make an assessment of the level of violence necessary to bring in central authorities and may keep below this level. In these cases the state, apparently absent, sets an upper bound on ethnic violence at a level that may prevent spirals and conceivably encourages in-group policing to avoid state intervention. In these cases, however, the state is only apparently absent because its interventions occur off the equilibrium path.

A second extension of the argument is to consider more fully the institutional implications of in-group policing. There are a number of points to explore. First, while we have argued that the quality of in-group policing arrangements may crucially affect the amount of interethnic violence, we have not elaborated a theory stating when effective in-group policing is most likely to arise (beyond the argument that relatively small groups are more likely to develop such arrangements). For that, we need to examine issues of collective action and political competition within ethnic groups. We then may better describe the conditions under which interethnic relations are most likely to remain peaceful, even off the equilibrium path. Second, we need to consider more fully the social and political consequences of in-group policing equilibria. We know that over time the millet and kahal systems broke down, with awful consequences for all groups but especially for minorities. *It is crucial that both spiral equilibria and in-group policing will tend to reproduce and maintain the sense of ethnic difference through time.* In addition, in-group policing may have the added liability that the same in-group institutions that prevent spiraling may be captured by ethnic entrepreneurs with an interest in fomenting ethnic violence and used by them to mobilize ethnic groups for conflict. The same in-group institutional structures used to identify and sanction members who offend against the other group can just as well be used to sanction members who seek to establish ties with ethnic others or who refuse to fight against them. While our analysis suggests that in-group policing is generally preferable to spiraling equilibria for maintaining interethnic cooperation, we are not recommending in-group policing as the best of all possible worlds. Finally, the risk of breakdowns in ethnic relations due to "noise" in individual interactions may be greater under in-group policing than under spiral equilibria, since the consequences can be much worse under the latter (i.e., all hell can break loose). Each of these considerations requires us to examine more fully in-group policing as a formal institution of governance.

How may an empirical research program develop along the lines indicated by the model? Our empirical section illustrated the plausibility of our assumptions and the existence of two distinct equilibria to address the problem of cauterizing interethnic violence once cooperation breaks down. With our model now available, it should be possible to devise more rigorous tests of its observable implications. First, our theoretical results suggest that the in-group policing equilibrium will have relatively greater cauterizing capacities compared to the spiral equilibrium when interethnic relations move off the equilibrium path. Second, our extension of the model in the case of uneven group size suggests that smaller groups are more likely to develop and more carefully monitor in-group policing schemes. Third, there is a rationale and interest for leaders of ethnic groups to limit interaction between coethnics and other groups, that is, to construct boundaries. Fourth, the problem of asymmetric information between groups opens up a market for local mediators who have specialized knowledge of the members (and personality types) in their group. More rigorous tests of these hypotheses remain on the agenda for future research.

Existing rationalist and psychological theories of ethnic conflict are premised on assumptions about group-level demands, grievances, and animosities. They tend to treat groups as actors or implicitly equate group motivation with that of representative members. In sharp contrast, we have attempted to develop a theory of interethnic conflict *and* cooperation that does neither. Instead, we have taken individual interactions subject to opportunism as our theoretical point of departure, showing how these can (1) provide a rationale for intraethnic institutions, some of which may play a role in maintaining "ethnicity" itself, and (2) create frictions and tension in interethnic interactions that nonetheless may be contained most of the time by two ideal types of equilibrium institutional arrangements. As stated in the introduction, we wish to stress that we do not view the approach developed here as a complete theory of ethnic cooperation and conflict. For example, we believe the more standard group-level analyses capture important aspects of the subject that our analysis, in its present form, does not. Furthermore, it is obvious that interethnic relations frequently involve powerful emotions which interact in complex ways with rational calculation. The rationalist analysis pursued here cannot easily address the role of affect in interethnic affairs. We offer the individual-interactions theory as a novel and potentially productive approach to understanding interethnic peace and violence, but not as the last word on the subject.

APPENDIX: EQUILIBRIUM CONDITIONS, PROOFS, AND EXPECTED PAYOFFS WITH NOISY INTERACTIONS

PROPOSITION 1: *Let* $\underline{\delta}_1 = \frac{\beta}{(1 - \mathrm{p})(1 + \beta)}, \underline{\delta}_2 = \frac{\alpha - 1}{(1 - \mathrm{p})(1 + \beta)}$, *and* $\underline{\delta}_{\mathrm{igp}} = \max\{\underline{\delta}_1, \underline{\delta}_2\}$. *Then,* σ_{igp} *forms a subgame perfect equilibrium of* G *if and only if* $\mathrm{p} < \min\left\{\frac{1}{1 + \beta}, 1 - \frac{\alpha - 1}{1 + \beta}\right\}$, *and* $\delta^{\mathrm{T^{igp}}} \geq \underline{\delta}_{\mathrm{igp}}$. *These conditions further imply both that* $\underline{\delta}_{\mathrm{igp}}$ *is the smallest discount factor that can support* σ_{igp} *as a subgame perfect equilibrium and that if* σ_{igp} *is subgame perfect for given* δ, p, *and other parameters, then it is always possible to choose* $\mathrm{T^{igp}} = 1$.

Proof. Consider i, a member of group A (which is sufficient since all the same arguments will apply to members of group B as well). Let the vector $s_t = (t_1, t_2, \ldots, t_n)$ summarize the state of the system at the beginning of period t, where t_i gives the number of periods remaining in player i's punishment phase, with the convention that $t_i = 0$ means that i is not in punishment phase. We will say that a player not in punishment phase is a "cooperator" and a player being punished is a "defector." Since σ_{igp} conditions only on the status of a player and her partner in each round (i.e., in-group/out-group, cooperator or defector), to establish subgame perfection (SGP) it will be sufficient to show that under the conditions in the proposition no player has an incentive to deviate from σ_{igp} in a single period for any state s_t (the optimality principle of dynamic programming is invoked here; see Fudenberg and Tirole 1991, 108–10, for a statement of the theorem and Milgrom, North, and Weingast 1990 for an application in a social matching game).

In particular, this means that we need to establish that for any s_t (1) a cooperator i has no incentive (a) to defect against an out-group player, (b) to defect against an in-group cooperator, and (c) to cooperate with an in-group defector; and (2) a defector i has no incentive (a) to defect against an out-group player, (b) to defect against an in-group cooperator, or (c) to cooperate against an in-group defector.

It is immediately evident that 1c and 2c will be satisfied for any s_t, any T^{igp}, and any δ, since in each case the proposed deviation strictly lowers the player's payoff in round t and then has no subsequent effects on any other player's strategy (these deviations are ignored). Furthermore, notice that a player has no incentive to deviate in case 1a if and only if she has no incentive to deviate in case 1b, since she faces exactly the same payoffs in period t and the same (equilibrium) profile of subsequent punishment in either case. So there are three conditions to check, 1a/1b, 2a, and 2b (for all states s_t).

Fix an s_t, and for integers $l \geq 0$ let n_{t+l} be the number of A-group players $j \neq i$ with $t_j \leq l$ (that is, the number of A players other than i who will not be in punishment phase in period $t + l$, if all follow σ_{igp}). Let $q_{t+l} = n_{t+l}/(n - 1)$, which is the probability that player i is paired with a cooperator in period $t + l$ if i is paired in-group.

To check condition 1a/1b, suppose a cooperator is paired with a cooperator or an out-group player in period t, when the state is s_t. Then, by defecting, player i gains $\alpha - 1$ in t while forgoing, in expected terms,

$$\sum_{l=1}^{T^{igp}} \delta^l (1 - p)(q_{t+l}(1 + \beta) + (1 - q_{t+l})\alpha). \qquad \textbf{(A-1)}$$

Note that $1 + \beta = 1 - (-\beta)$ is what i has forgone if i is paired with a cooperator, and $\alpha = \alpha - 0$ is what i has forgone if i is paired with a defector.

SGP requires that $\alpha - 1$ be less than or equal to expression A-1 for *any* feasible state s_t. This means that SGP will hold only if $\alpha - 1$ is weakly less than this expression for the feasible s_t that implies the sequence $(q_{t+1}, q_{t+2}, \ldots)$ that minimizes expression A-1. Clearly, if $\alpha > 1 + \beta$ then expression A-1 is minimized by $q_{t+l} = 1$ for $1 \leq l \leq T^{igp}$, which is feasible since it occurs when all A players are cooperators in period $t - 1$. Thus, if $\alpha > 1 + \beta$, then SGP requires

$$\delta \frac{1 - \delta^{T^{igp}}}{1 - \delta} \geq \frac{\alpha - 1}{(1 - p)(1 + \beta)},$$

or

$$\delta^{T^{igp}} \leq 1 - \underline{\delta}_2 \frac{1 - \delta}{\delta}. \qquad \textbf{(A-2)}$$

If $\alpha \leq 1 + \beta$, however, then expression A-1 is minimized by $q_{t+l} = 0$ for $1 \leq l \leq T^{igp} - 1$ and $q_{t+T^{igp}} = 1$. The following argument justifies this claim: For all states s_t, it must be that $q_{t+T^{igp}} = 1$, since if all players $j \neq i$ play according to σ_{igp} from period t forward (as i assumes when deciding whether to deviate in period t), then no $j \neq i$ can be in punishment phase in period $t + T^{igp}$. Thus, the minimum value for expression A-1 occurs in the (feasible) state s_t, where all A players were cooperators in period $t - 2$, and all except i defected in period $t - 1$, which renders $q_{t+l} = 0$ for $1 \leq l \leq T^{igp} - 1$.[50] Thus, in this case SGP requires that

$$\alpha - 1 \leq \left(\sum_{l=1}^{T^{igp}-1} \delta^l (1 - p)\alpha \right) + \delta^{T^{igp}}(1 - p)(1 + \beta). \qquad \textbf{(A-3)}$$

To check 2a, suppose a defector i (thus, $0 < t_i \leq T^{igp}$) is paired with an out-group player in period t. By defecting, he gains $\alpha - 1$ while forgoing

$$\sum_{l=1}^{t_i - 1} \delta^l (1 - p)(q_{t+l}0 + (1 - q_{t+l})0)$$

$$+ \sum_{l=t_i}^{T^{igp}} \delta^l (1 - p)(q_{t+l}(1 + \beta) + (1 - q_{t+l})\alpha).$$

For any s_t (less the component t_i), this expression is minimized by $t_i = T^{igp}$, leaving the requirement that for all s_t

$$\alpha - 1 \leq (1 - p)\delta^{T^{igp}}(q_{t+T^{igp}}(1 + \beta) + (1 - q_{t+T^{igp}})\alpha).$$

But for any s_t, $q_{t+T^{igp}} = 1$, since if all play according to σ_{igp}, then all players $j \neq i$ are cooperators T^{igp} periods hence. Thus, SGP requires that $\delta^{T^{igp}} \geq \frac{\alpha - 1}{(1 - p)(1 + \beta)}$.

To check 2b, suppose a defector i is paired with an in-group cooperator in period t. She gains β by defecting, while forgoing

$$\sum_{l=1}^{t_i - 1} \delta^l 0 + \sum_{l=t_i}^{T^{igp}} \delta^l (1 - p)(q_{t+l}(1 + \beta) + (1 - q_{t+l})\alpha).$$

For any s_t (less t_i) this expression is minimized by $t_i = T^{igp}$, which yields

$$\beta \leq \delta^{T^{igp}}(1 - p)(q_{t+T^{igp}}(1 + \beta) + (1 - q_{t+T^{igp}})\alpha).$$

And since $q_{t+T^{igp}} = 1$, we have the condition $\delta^{T^{igp}} \geq \frac{\beta}{(1 - p)(1 + \beta)}$. Combining this with the condition for 2a yields

$$\delta^{T^{igp}} \geq \max\left\{ \frac{\beta}{(1 - p)(1 + \beta)}, \frac{\alpha - 1}{(1 - p)(1 + \beta)} \right\} = \max\{\underline{\delta}_1, \underline{\delta}_2\}. \qquad \textbf{(A-4)}$$

Considering expressions A-2, A-3, and A-4, there are now two cases to consider. In the first, if $\alpha \leq 1 + \beta$, then SGP requires that $\delta^{T^{igp}} \geq \underline{\delta}_1$ and expression A-3 hold. Since $\delta^{T^{igp}}$ is decreasing in T^{igp}, $\delta \geq \underline{\delta}_1$ is necessary for the former to be satisfied. It also proves to be sufficient to guarantee expression A-3 as well, since if we let $T^{igp} = 1$, then we can rearrange expression A-3 to yield $\delta \geq \frac{\alpha - 1}{(1 - p)(1 + \beta)} = \underline{\delta}_2$, which is assured by $\delta \geq \underline{\delta}_1$, since $\alpha \leq 1 + \beta$ implies $\underline{\delta}_1 \geq \underline{\delta}_2$.

In the second case, where $\alpha > 1 + \beta$, SGP requires that

$$\underline{\delta}_2 \leq \delta^{T^{igp}} \leq 1 - \underline{\delta}_2 \frac{1 - \delta}{\delta}.$$

An analogous argument applies, and the conditions given in the proposition follow immediately. (The condition on p is necessary in order for the larger of $\underline{\delta}_1$ and $\underline{\delta}_2$ to be less than one.) *Q.E.D.*

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[50] Note that under σ_{igp}, simultaneous defections are not ignored.

PROPOSITION 2: *(i) For cases in which* $\alpha \le 1 + \beta$, *let* $\underline{\delta}_S = \max \left\{ \underline{\delta}_1, \frac{\alpha - 1}{\alpha - 1 + p} \right\}$. *Then* σ_S *is subgame perfect if and only if* $\frac{(\alpha - 1)(1 - \delta)}{\delta} \le p < \frac{1}{1 + \beta}$, $\delta \ge \underline{\delta}_S$, $\delta^{T^{in}} \ge \underline{\delta}_1$, *and* $T^{out} \ge \ln\left(1 - \frac{\alpha - 1}{p}\frac{1 - \delta}{\delta}\right) / \ln \delta$. *(ii) For cases in which* $\alpha > 1 + \beta$, *let* T^{in}_{min} *be the smallest integer greater than or equal to*

$$\frac{\ln \underline{\delta}_1}{\ln\left(\frac{\underline{\delta}_2}{\underline{\delta}_2 + 1 - \underline{\delta}_1}\right)},$$

and let $\hat{\underline{\delta}} = \underline{\delta}_1^{1/T^{in}}$. *Then* σ_S *forms a subgame perfect equilibrium of* G *if and only if*

$$\frac{(\alpha - 1)(1 - \delta)}{\delta} \le p < \frac{1}{1 + \beta}, \; \delta \ge \max\left\{ \hat{\underline{\delta}}, \frac{\alpha - 1}{\alpha - 1 + p} \right\},$$

$$T^{in} \ge T^{in}_{min}, \text{ and } T^{out} \ge \ln\left(1 - \frac{\alpha - 1}{p}\frac{1 - \delta}{\delta}\right) / \ln \delta.$$

In case (i), T^{in} *can be chosen to equal one, while* T^{in}_{min} *is greater than one in case (ii).*

Proof. Consider a member i of group A (again, the same arguments apply for members of B). Let $s_t = (t_0, t_1, t_2, \ldots, t_n)$ represent the state of the system as before, except that t_0 is the number of periods remaining in the out-group punishment phase at the end of period $t - 1$, where $t_0 = 0$ means that neither group is being punished. Parallel to the in-group policing case above, to establish subgame perfection we need to show that for any state s_t, (1) a cooperator i has no incentive (a) to defect in an out-group pairing if $t_0 = 0$, (b) to cooperate in an out-group pairing if $t_0 > 0$, (c) to defect in an in-group pairing with a cooperator if $t_0 = 0$ and if $t_0 > 0$, and (d) to cooperate with an in-group defector when $t_0 = 0$ and $t_0 > 0$; and (2) a defector i has no incentive (a) to defect in an out-group pairing if $t_0 = 0$, (b) to cooperate in an out-group pairing if $t_0 > 0$, (c) to defect in an in-group pairing with a cooperator when $t_0 = 0$ and when $t_0 > 0$, and (d) to cooperate with an in-group defector when $t_0 = 0$ and when $t_0 > 0$.

In cases 1b, 1d, 2b, and 2d, the proposed deviation from σ_S lowers the deviator's payoff in period t but has no subsequent effects, so these are clearly not optimal for any s_t, T^{in}, T^{out}, and δ. We proceed to find conditions on δ, T^{in}, and T^{out} such that 1a, 1c, 2a, and 2c hold.

Define q_{t+l} exactly as before. For the case 1a, suppose a cooperator i is paired with a B player in a period t when $t_0 = 0$. Defecting yields a gain of $\alpha - 1$ in period t while i forgoes $\delta p(1 - 0) + \delta^2 p(1 - 0) + \ldots + \delta^{T^{out}} p(1 - 0)$, since under σ_S a deviation in an out-group pairing has no effect on a player's payoff in in-group pairings. This yields the condition

$$\delta \frac{1 - \delta^{T^{out}}}{1 - \delta} \ge \frac{\alpha - 1}{p}. \qquad \text{(A-5)}$$

For case 1c, suppose a cooperator is paired in-group with another cooperator and with s_t such that $t_0 = 0$. Then he faces exactly the same expected payoffs for C versus D as in case 1b of the in-group policing proof above. Similarly, if $t_0 > 0$, then what is gained by defecting versus what is lost is the same as in case 1b of the in-group policing case. Thus, the conditions for SGP are given by inequality A-2 when $\alpha > 1 + \beta$, and inequality A-3 when $\alpha \le 1 + \beta$, substituting T^{in} for T^{igp}.

For case 2a, where a defector i is paired with an out-group player when $t_0 = 0$, i faces exactly the same trade-offs as in case 1a (of the spiral equilibrium case), so we have expression A-5 again. Finally, for case 2c (a defector paired in-group with a cooperator when $t_0 = 0$ and when $t_0 > 0$), defecting yields a gain of β while i forgoes $\Sigma_{l=t_i}^{T^{in}} \delta^l (1 - p)(q_{t+l}(1 + \beta) + (1 - q_{t+l})\alpha)$ whether $t_0 = 0$ or not. Thus, the logic of case 2b for σ_{igp} applies, yielding the condition

$$\delta^{T^{in}} \ge \frac{\beta}{(1 - p)(1 + \beta)}. \qquad \text{(A-6)}$$

Putting these conditions together, there are now two cases. When $\alpha \le 1 + \beta$, the conditions for δ and T^{in} are the same as for δ and T^{igp} in the parallel case at the end of the proof of proposition 1, with the additional restriction that $\frac{\delta}{1 - \delta} \ge \frac{\alpha - 1}{p}$, or $\delta \ge \frac{\alpha - 1}{\alpha - 1 + p}$, so that it is possible to deter defection in out-group pairings under the strongest possible punishment ($T^{out} = \infty$).

In the second case, when $\alpha > 1 + \beta$, SGP requires that

$$\underline{\delta}_1 \le \delta^{T^{in}} \le 1 - \underline{\delta}_2 \frac{1 - \delta}{\delta}. \qquad \text{(A-7)}$$

Note that if $p < 1/(1 + \beta)$ (so that $\underline{\delta}_1 < 1$), we can guarantee that this will hold by choosing δ close enough to one and T^{in} large enough. The problem is to find the minimum δ such that it holds for some choice of T^{in}. Observe that $\delta \ge \underline{\delta}_1$ is necessary because $\delta^{T^{in}}$ decreases with T^{in}. But it is not sufficient, since $\delta = \underline{\delta}_1$ implies $\underline{\delta}_1 \le 1 - \underline{\delta}_2 \frac{1 - \underline{\delta}_1}{\underline{\delta}_1}$, which reduces to $\underline{\delta}_2 \le \underline{\delta}_1$, a false statement when $\alpha > 1 + \beta$.

The minimum δ in this case, call it $\hat{\underline{\delta}}$, must satisfy $\underline{\delta}_1 = \hat{\underline{\delta}}^{T^{in}}$ for some T^{in}, since otherwise it would be possible to lower $\hat{\underline{\delta}}$ without violating either inequality of expression A-7. Thus, $\hat{\underline{\delta}} = \underline{\delta}_1^{1/T^{in}}$, and SGP requires that we choose the minimum T^{in} such that

$$\underline{\delta}_1 \le 1 - \underline{\delta}_2 \frac{1 - \underline{\delta}_1^{1/T^{in}}}{\underline{\delta}_1^{1/T^{in}}}. \qquad \text{(A-8)}$$

Algebra can then be used to extract the conditions for δ, T^{in}, and T^{in}_{min} given in the proposition. The stated requirement on T^{out} derives from expression A-5 and is needed so that there is no incentive to defect in an out-group pairing, and the condition that $\delta \ge \frac{\alpha - 1}{\alpha - 1 + p}$ (equivalently, $p \ge \frac{(\alpha - 1)(1 - \delta)}{\delta}$) is necessary for there to exist such a T^{out}. The condition $p < \frac{1}{1 + \beta}$ is necessary for $\underline{\delta}_1 < 1$, which is in turn necessary for expression A-7 to be possible. *Q.E.D.*

Claim. When $\alpha \le 1 + \beta$, σ_{igp} is less restrictive than σ_S in the sense that whenever σ_S forms a SGP equilibrium then so does σ_{igp}, while σ_{igp} may be SGP for parameter values such that σ_S is not. When $\alpha > 1 + \beta$, for given parameter values it may be possible to support both, neither, or only one of σ_{igp} and σ_S as SGP equilibria.

Proof. This follows immediately from propositions 1 and 2 in cases where $\alpha \le 1 + \beta$, since the conditions for SGP are the same except that σ_S also requires that $\delta \ge \frac{\alpha - 1}{\alpha - 1 + p}$. For cases with $\alpha > 1 + \beta$, the claim is directly implied by the fact that the upper bound on p is larger under σ_S than under σ_{igp}, while the lower bound on p is smaller under σ_{igp} than under σ_S (see propositions 1 and 2). *Q.E.D.*

Expected Payoffs with Noise in Interethnic Interactions

We assume that the noise takes the following form. Players intending to cooperate "accidentally" defect with probability $\varepsilon > 0$ in interethnic pairings. Defection always produces defection. Note that under σ_{igp} and σ_S mutual defections in interethnic pairings are not ignored, so that DD triggers a spiral under σ_S and in-group punishment of both players under σ_{igp}. These assumptions are made simply to reduce the complexity of the calculations and are not crucial for the results reported in the text.

Noise in the spiral equilibrium. If σ_S forms an equilibrium given a level of noise $\varepsilon > 0$, then a player's ex ante expected equilibrium payoff, call it V_S^*, can be computed as follows. The expected payoff in period t if paired with an ethnic other is $B = (1 - \varepsilon)^2 + (\alpha - \beta)\varepsilon(1 - \varepsilon)$. V_S^* is then defined by the recursive equation

$$V_S^* = pB + (1 - p) + \delta\Big((1 - \varepsilon)^{2k} V_S^* + (1 - (1 - \varepsilon)^{2k}) \cdot \left[(1 - p)\frac{1 - \delta^{T^{out}}}{1 - \delta} + \delta^{T^{out}} V_S^* \right] \Big),$$

where $pB + (1 - p)$ is expected payoff in the current round, $(1 - \varepsilon)^{2k}$ is the probability that no one defects in an interethnic pairing, and the expression in brackets is expected payoff if cooperation with the out-group breaks down. This solves to

$$V_S^* = \frac{pB + (1 - p) + \delta(1 - p)\dfrac{1 - \delta^{T^{out}}}{1 - \delta}(1 - (1 - \varepsilon)^{2k})}{1 - \delta(\delta^{T^{out}} + (1 - \delta^{T^{out}})(1 - \varepsilon)^{2k})}.$$

For σ_S to remain an equilibrium with ε noise, it is necessary that a player's expected payoff for cooperating when paired with an ethnic other (call it $V_S|C$) be greater than or equal to the payoff for defecting against an ethnic other ($V_S|D$). $V_S|C$ and $V_S|D$ are computed as follows:

$$V_S|C = B + \delta\Bigg((1 - \varepsilon)^{2k}V_S^* + (1 - (1 - \varepsilon)^{2k})$$
$$\cdot \left[(1 - p)\frac{1 - \delta^{T^{out}}}{1 - \delta} + \delta^{T^{out}}V_S^*\right]\Bigg),$$

and

$$V_S|D = (1 - \varepsilon)\alpha + \delta\left((1 - p)\frac{1 - \delta^{T^{out}}}{1 - \delta} + \delta^{T^{out}}V_S^*\right)$$

Using a computer, one can then calculate the minimum T^{out} such that σ_S can form a Nash equilibrium given ε noise and other parameter values, and also the ex ante expected value of the equilibrium for a player, V_S^*.

For cases in which $\alpha - \beta < 1$, it is straightforward to show that Nash equilibrium and subgame perfection with $\varepsilon > 0$ require no conditions additional to the one just given and to those stated in proposition 2(i). Consider a player i paired in-group in period t. The trade-offs she faces for defecting versus cooperating are exactly those given for case 1c in the proof of proposition 2, since whether she defects in-group has no effect on what happens in subsequent (noise-affected) outgroup relations. That is, expected payoffs from outgroup interactions will be the same regardless of one's behavior within the group, so the in-group conditions for the case in which $\varepsilon = 0$ apply. Next, concerning out-group pairings and off-equilibrium-path states s_t, note that when $\alpha - \beta < 1$ we can always satisfy the conditions for in-group propriety by setting $T^{in} = 1$. Thus, if one is paired outgroup in period t, one expects that all coethnics will be in cooperation status in $t + 1$. This implies that regardless of the state s_t (but with $t_0 = 0$), $V_S|C$ and $V_S|D$ are exactly as given above. This demonstrates subgame perfection, since the remaining cases (1b, 1d, 2b, and 2d in the proof of proposition 2) are again trivially satisfied.

For the case of $1 < \alpha - \beta < 2$ (which is less empirically relevant when defection means violence, since one then would expect β to be much greater than α), T^{in}_{min} is greater than one, and the expressions for the expected utility of cooperating versus defecting with $\varepsilon > 0$ become extremely complicated. We have analyzed the case of $T^{in} = 2$, which proves to reduce (for any s_t with $t_0 = 0$) to the same condition as that given immediately above. We conjecture that the same would be true for any T^{in}, but we have not shown this.

Noise under in-group policing. Let $B = (1 - \varepsilon)^2 + (\alpha - \beta)\varepsilon(1 - \varepsilon)$ as before. For notational convenience, let $K = \dfrac{k}{n - 1}$ be the fraction of A-group players other than i who were paired out-group in the last period if i was paired in-group in that period, and let $K_{-1} = \dfrac{k - 1}{n - 1}$ be the fraction of A-group players other than i who were paired out-group in the last period if i was paired out-group. Suppose provisionally that $T^{igp} = 1$, which is always possible when σ_{igp} forms an equilibrium with $\varepsilon = 0$ (see proposition 1). Then player i's ex ante expected equilibrium payoff is $V_{igp}^* = pB + (1 - p) + \delta\dfrac{\hat{V}_{igp}^*}{1 - \delta}$, where $\hat{V}_{igp}^*$ is defined as

$$\hat{V}_{igp}^* = pB + (1 - p)[p\varepsilon(K_{-1}\varepsilon 0 + (1 - K_{-1}\varepsilon)(-\beta)) + p(1 - \varepsilon)$$
$$(K_{-1}\varepsilon\alpha + 1 - K_{-1}\varepsilon) + (1 - p)(K\varepsilon\alpha + 1 - K\varepsilon)].$$

The expression within brackets gives probabilities and expected payoffs for three events that may have occurred in the prior period. There is a $p\varepsilon$ chance that i paired out-group and defected, a $p(1 - \varepsilon)$ chance that i paired out-group and cooperated, and a $1 - p$ chance that i paired in-group. Payoffs in each case depend on the probability of being paired with someone who defected in an out-group pairing in the prior period—for example, $K_{-1}\varepsilon$ is the probability that i meets a defector if i paired out-group in the last period.

Related expressions may then be derived for $V_{igp}|C$, i's expected payoff if i chooses to cooperate in an out-group pairing, and $V_{igp}|D$, i's expected payoff if i deliberately defects in an out-group pairing. These are

$$V_{igp}|C = B + \delta[pB + (1 - p)(-\varepsilon\beta(1 - K_{-1}\varepsilon)$$
$$+ (1 - \varepsilon)(K_{-1}\varepsilon\alpha + 1 - K_{-1}\varepsilon))] + \frac{\delta^2\hat{V}_{igp}^*}{1 - \delta}$$

and

$$V_{igp}|D = (1 - \varepsilon)\alpha + \delta[pB + (1 - p)(K_{-1}\varepsilon 0$$
$$+ (1 - K_{-1}\varepsilon)(-\beta))] + \frac{\delta^2\hat{V}_{igp}^*}{1 - \delta}.$$

V_{igp}^* can then be computed as above, subject to the condition that $V_{igp}|C \geq V_{igp}|D$. In contrast to the spiral equilibrium with noise, however, in this case we need to revise the equilibrium conditions for in-group pairings, because under in-group policing noise in interethnic pairings affects the expected number of players to be punished *within* the group. Consider first the case of a cooperator paired in-group with another cooperator. Provisionally assuming that we can support an SGP equilibrium with $T^{igp} = 1$, cooperating yields

$$1 + \delta(pB + (1 - p)[K\varepsilon\alpha + 1 - K\varepsilon]) + \delta^2\frac{\hat{V}_{igp}^*}{1 - \delta},$$

while defecting yields

$$\alpha + \delta(pB + (1 - p)[K\varepsilon 0 + (1 - K\varepsilon)(-\beta)]) + \delta^2\frac{\hat{V}_{igp}^*}{1 - \delta}.$$

The former is greater than the latter provided that

$$\delta \geq \frac{\alpha - 1}{(1 - p)(K\varepsilon\alpha + (1 - K\varepsilon)(1 + \beta))},$$

which is virtually the same as $\underline{\delta}_2$ for small ε. A parallel argument establishes that a defector paired in-group with a cooperator has no incentive to defect provided that

$$\delta \geq \frac{\beta}{(1 - p)(K\varepsilon\alpha + (1 - K\varepsilon)(1 + \beta))},$$

which is approximately equal to $\underline{\delta}_1$ for small ε. Note that since $T^{igp} = 1$, these expressions apply regardless of the state of the system s_t, since if all follow σ_{igp} in period t, then all except those who accidentally defect in cross-group pairings will be in cooperation status in period $t + 1$. Thus, if these conditions and $V_{igp}|C \geq V_{igp}|D$ are satisfied, then σ_{igp} chosen by all players can form an SGP with $\varepsilon > 0$ noise in interethnic interactions. (The analogue to condition 2a in the proof of proposition 1 is satisfied when T^{igp} is set equal to one, since a defector in period $t - 1$ paired out-group in period t is then in the same situation as a cooperator paired out-group in period t. The analogues to conditions 1c and 2c are again trivially satisfied.)

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The Electoral Connection in the Chinese Countryside

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A 1987 law established popularly elected village committees in the Chinese countryside. This article analyzes a unique set of survey data to describe and explain the connection between village leaders and those who choose them, in terms of orientation to the role of the state in the economy. It compares positions of village leaders with positions of respondents sampled from their selectorates of township-level leaders and electorates of ordinary villagers. Results of multivariate regression analyses indicate that: (1) village leaders are responsive to both old and newly emerging constituencies, as reflected in significant congruence between village leaders and their selectorates above and electorates below; (2) congruence between village leaders and their electorates is not exclusively the result of shared local environment, informal influence, or socialization but is significantly associated with the electoral process; and (3) the causal mechanism underlying the electoral connection in the Chinese countryside is the familiar one of voter choice.

Quantitative empirical inquiry into congruence between political leaders and ordinary citizens takes as its point of departure the following claim: Leaders emerge as the outcome of a competitive political process influenced in large part by preferences of voters for particular policies. Analysis of congruence between leaders and citizens evaluates the quality of representation by measuring the extent to which the electoral connection produces a policy connection, usually as reflected in significant agreement between citizens and elected leaders or governing parties on specific issues or a general ideological dimension (see especially Achen 1978, Barnes 1977, Converse and Pierce 1986, Dalton 1985, Huber and Powell 1994, Miller and Stokes 1963, Powell 1982). By some measure, leaders in liberal democracies are supposed to be close to the center of their constituencies.

Without the theoretical premise of an electoral connection, there is little to motivate analysis of congruence. That explains the lack of comparable studies in communist states. Indeed, the key mechanism connecting leaders and citizens in those states is not designed to transform mass preferences into public policy (see Dahl 1989, Lindblom 1977, Meyer 1957). Put simply, the basic theory sustaining the communist claim to legitimate authority presumes that communist leaders are representative in the following sense: Communist Party members comprise an enlightened revolutionary vanguard that alone is able to comprehend and represent the objective long-term interests of the masses. Depending on the level of economic development and mass revolutionary consciousness, however, those interests may not correspond closely to some aggregation of preferences of ordinary citizens. Accordingly, what guarantees that communist leaders represent mass interests is not regular electoral competition but the effective monopoly of power by a hierarchically organized, internally disciplined, politically correct Communist Party elite (Lenin 1902, 1904, 1919).

Elections in communist states are not, then, in conception or practice, public policy contests. Nor does the Leninist framework allow electoral challenges to the leading role of the Communist Party; elections are organized as fully predictable plebiscites or limited-choice elections (see Pravda 1978) in which the range of outcomes is restricted to candidates vetted and approved by Communist Party selectorates at higher levels. Such an arrangement suggests congruence between elected leaders and their selectorates, rather than electorates, but conditions for political survey research in communist states have generally not been conducive to inquiry into congruence of any sort.

Recent changes in economic and political organization in the Chinese countryside are producing a new basis for inquiry into congruence between leaders at the rural grassroots and those who choose them—not only their selectorates of leaders at the higher township level but also their electorates of ordinary villagers. At the same time, improved circumstances for research have made such an inquiry possible. This article draws on a unique set of survey data to analyze the connection between village leaders and their selectorates and electorates in terms of congruence of positions on a basic question of political economy: the importance of state management relative to private individual initiative in economic development.

The data analyzed here are from a sample of 56 Chinese villages surveyed in 1990, after passage in 1987 of a draft law establishing popularly elected village committees as autonomous organizations of power in the countryside. The analysis exploits two features of the data set that form a unique combination in survey research on mainland China: (1) a sampling design that nests the villages within 20 townships and four counties (see Appendix A), which permits matching village leaders with those who select and elect them, and (2) the commonality of many questionnaire items in elite and mass interviews, which permits matching responses to the same items. The analysis of congruence compares positions of village leaders on the role of the state in the economy with positions of respondents sampled from their township selectorates and village electorates.

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For the most part, research on the new politics in the Chinese countryside has focused on informal processes and relationships (see especially Oi 1986, 1989; O'Brien and Li 1995). The relative lack of attention to formal politics is understandable. Meaningful political reform has lagged far behind economic reform, with many purportedly grand changes in political arrangements implemented only superficially or abandoned after experimentation. In addition, the unchanged official commitment to a Leninist party monopoly of power appears inconsistent with a program of grassroots democratization.

This article describes and explains how peasants in one of the world's few remaining communist states are influential actors in the formal politics of elections. It contributes three important—and, in context, by no means obvious—new empirical findings to our knowledge about elite-mass linkages in the Chinese countryside. First, it provides evidence of responsiveness to both old and newly emerging constituencies, reflected in significant congruence between village leaders and their selectorates above and electorates in the villages. Second, it demonstrates that congruence between village leaders and their electorates is not exclusively the result of shared local environment, informal influence, or socialization but is significantly associated with institutions of rural grassroots democratization. Third, it suggests that the causal mechanism underlying the electoral connection in the Chinese countryside is a familiar one: Voters with more choice can choose candidates closer to them in orientation.

THE NEW CONTEXT OF VILLAGE LEADERSHIP

Agricultural decollectivization dismantled rural communes and their subordinate production units, replacing them over a few years with township governments and village committees. Land and other production inputs were divided among peasant households to manage on their own, free markets were reopened, most obligatory sales to the state were abolished, and private entrepreneurship was promoted. The new incentive structure for agricultural production quickly proved successful by most economic standards, but its consequences for leadership at the rural grassroots were very serious. Decollectivization enormously weakened the state's hold over village leaders, the dependence of villagers on those leaders, and, as a result, the power of the state to exact compliance from peasants (see Unger 1989). To manage villages as decollectivization progressed, the 1982 Constitution introduced "autonomous village committees," but the official effort had essentially ended there. By the mid-1980s, village leadership had atrophied: Village meetings were rarely convened; peasants resisted efforts by village leaders to implement such unpopular directives as family planning; tensions erupting in violent conflicts between villagers and village leaders became common; and many village leaders abdicated positions of leadership to enrich themselves as specialized producers or entrepreneurs, while others stayed in their positions to enrich themselves through illicit means (see especially White 1992, Zheng 1989).

As a response to the deterioration of organizations and leadership at the rural grassroots, the National People's Congress passed a draft law, effective June 1988, to reform village committees. Somewhat paradoxically, the law was designed to enhance state capacity to govern in the countryside through grassroots democratization to increase accountability of village leaders to villagers. The underlying idea was that villagers would be more responsive to leaders chosen from below rather than imposed from above. The law defined village committees as mass organizations of self-government at the grassroots, popularly elected and accountable to a village council comprised of all adult villagers. These committees of three to seven members, elected to three-year terms, were to help township governments carry out their work. Significantly, the law did not place committees under the leadership of the township governments or local party organizations (National People's Congress 1987).

Offering villagers electoral choice was a key component of the grassroots democratization measures embodied in the law; official guidelines called for village committee elections to feature some choice among candidates. By the end of 1990, rudimentary elected village committees had by and large replaced the old committees. Most villages held semicompetitive elections, featuring one or two more candidates than the number of positions, and some villages experimented with fully competitive elections, listing twice the number of candidates as positions, but many continued the old practice of elections without choice (see O'Brien 1994).

IMPLICATIONS FOR CONGRUENCE: SELECTORATES AND ELECTORATES

As Chinese officials acknowledge, grassroots democratization is a long-term process; it began not long ago, and progress has been uneven. Many village committee elections are uncompetitive, most only semicompetitive. Practically none feature campaigns. Overlapping membership on village Communist Party branches and village committees is common. Moreover, leaders at the township level, who are charged officially with organizing village elections, have an interest in maintaining the hierarchy of control over villages. What, then, are the real implications of village elections for congruence at the rural grassroots?

Consider the implications for congruence between village leaders and their selectorates. Although village leaders are not state officials, and, therefore, the superior party committee does not have formal personnel authority over those positions, party secretaries and government heads at the township level function nonetheless as selectorates. Election outcomes offer two indications that township leaders do not leave village elections entirely to the vagaries of electorates: Village committees usually have representative quotas of women (and minorities where relevant), and elected village leaders are usually also village party branch leaders (Du 1990, Liaoning Provincial Department of Civil Affairs 1989, Zhang 1992). Township party and government leaders have a number of ways to influence the selection process in village elections. In extraordinary circumstances, if village committees are "paralyzed," township governments have the authority (indeed, the duty) to dismantle them, conduct investigations, and ensure that capable village leaders are elected, which can involve sending in their own candidates from outside (Chen 1990, Cui and Zhang 1990, Shi 1990). More commonly, township governments and party committees function as selectorates by influencing candidate nomination and monopolizing candidate vetting. In selecting candidates, township leaders consider the performance of village incumbents and rely on suggestions from the village party branches. Candidate nomination is also supposed to include a process of "deliberation" and "consultation," in which villagers are mobilized in groups to suggest and discuss potential candidates. Township leaders play a more direct role as selectorates in the process of vetting potential candidates and reducing the number on the ballot to one or a few more candidates than positions (see especially Fujian Provincial Department of Civil Affairs 1990). In some villages the elections themselves are organized so as to restrict free choice—for example, through the use of nonsecret ballots or voting by clapping or raising hands (see Kelliher 1996).

Township selectorates find it in their interest nonetheless to pay attention to electoral preferences of villagers. Certainly, township leaders fear that village leaders will be responsive to the peasants who elect them, at the expense of higher level authorities who rely on them as their "legs" in grassroots implementation (Shi 1990). But village leaders who must resort to coercive measures to enforce compliance with unpopular state policies are often ultimately unable to meet policy objectives (Zhang 1992, Zheng 1989). Such leaders may not initially lose support at the township level, but they do lose village elections (see, e.g., Yueping County 1990). Township leaders can fail as selectorates if they incorrectly assess village electoral support for candidates. Failure after elections can be more consequential yet: If elected village leaders cannot elicit cooperation from villagers, then township leaders effectively lose their "legs."

For other reasons, too, congruence between village leaders and their selectorates does not necessarily preclude congruence between village leaders and their electorates. Chinese villages are relatively small—the average size in 1990 was about 1,200—and most adult villagers have lived and worked together all their life. Even without formal campaigns and long before the electoral process begins, villagers usually know candidates personally and are familiar with their views. White (1992) observes that because village leaders (and their families) remain in the community after their term is up, leaders are typically reluctant to make enemies in the village while in office. In short, the familiarity in a small community provides a basis for choosing village leaders with congruent views and also creates incentives for their responsiveness to villagers.

Moreover, although some townships have organized uncompetitive elections, essentially continuing the old practices of direct appointment and dismissal (Zhang and Xiao 1992), most townships are weaker in their capacity as selectorates. Selectorates can influence who appears on the ballots, but winning votes in semicompetitive elections is usually the only way village leaders can gain office. Many provinces require that they win by a majority of eligible voters (see, e.g., Fujian Provincial People's Congress 1990). Villagers have withheld votes for candidates supported by the township and successfully resisted township efforts to dismantle "paralyzed" committees. Candidates who do not measure up in the political vetting process have been elected as village leaders (Li 1990). Peasants have petitioned higher level authorities to protest electoral violations of the law on village committees (O'Brien and Li 1995).

In sum, despite the new context of village leadership, there remains sufficient basis to expect congruence between village leaders and their township selectorates. Despite a continued official commitment to Leninism, however, there are new reasons to expect congruence between village leaders and villagers.

MEASURING CONGRUENCE: THE ROLE OF THE STATE IN THE ECONOMY

The state's role in the economy is a key policy issue in countries long dominated by central planning and state ownership. For the Chinese, already in their second decade of economic reform, it has become an issue of widespread relevance. While the Chinese have not privatized state enterprises, a main thrust of economic reform has been to promote the growth of collective and private enterprises so as to reduce the proportionate size of the state sector. Despite some antimarket orientation among conservative leaders, official policy in the reform period has promoted the private sector (Young 1991). In 1989 an estimated one in seven households in the countryside owned a private enterprise (Odegaard 1992).

The appropriate role of the state relative to private individual initiative, as the agent to promote and manage economic development, is typically treated as a key component of ideological orientation in liberal democracies, with positions on the issue corresponding to positions on a left-right dimension (see Huber 1989). It is also a suitable focus for analysis of congruence between village leaders and those who choose them. Village leaders have many responsibilities, but promoting economic development and creating an environment in which villagers can enrich themselves are among their most important (Zhang 1992). Village leaders act as economic managers of small, multidimensional businesses, adapting economic directives from above to local conditions (see Rozelle 1994). In providing economic leadership, village leaders have a range of choices reflecting fundamentally different orientations to the basic issue of the economic role of the state relative to private individual initiative. By contrast, state assignments such as family planning, labor conscription, and grain procurement, which undoubtedly shape the relationship between leaders and villagers in important ways, offer village leaders comparatively less scope for innovative implementation. They are fairly rigid obligations handed down the hierarchy by township governments.

To measure views on the economic role of the state, I used two questions to evaluate the importance of state and individual private enterprises:

> Here is a scale. The "0" on the scale indicates no importance and the "10" indicates extreme importance. How important would you say state enterprises are for economic development? How important would you say individual [i.e., private] enterprises are for economic development?

The broad interpretation of responses adopted here (for which I provide some empirical support below) considers them as positions on an ideological dimension that taps general orientation to the role of the state in the economy.[1] A respondent's position on the dimension is constructed by subtracting the reported position on the importance of individual enterprises from the reported position on the importance of state enterprises. Employing responses to both questions (which share the same wording and scale) has the advantage of taking into account idiosyncratic differences in implicit scales for evaluation. For the ordinary villagers, village leaders, and township leaders surveyed, Table 1 reports separately the frequency distributions for positions on the dimension.

Interpreted narrowly, positions are evaluations of the additional importance of state enterprises for economic development, relative to individual private enterprises. Positive positions assign greater importance to state enterprises than to individual enterprises. For this sample, that is by far the prevailing view in all three groups of respondents. Negative positions, by contrast, reflect the view that individual enterprises are relatively more important. A position of zero assigns equal importance to the two forms of enterprises.

Even interpreted narrowly, positions on the dimension are of obvious intrinsic interest. There is also some empirical support for a presumption that the dimension reflects more than isolated views on a specific question about enterprises. Respondents were asked a number of questions designed to tap views on the economic reforms of the 1980s. They were asked to indicate agreement or disagreement (on a four-point scale) with assertions about the effects of the economic reforms generally and, more specifically, about open markets, individual enterprises, and income inequality. They were also asked to indicate feelings (on a feeling thermometer) about private retailers and households that had prospered greatly under the rural economic reforms.[2] If responses to the questions on the importance of state and individual enterprises are more broadly interpretable as orientations toward the state's role in the economy, then they should be associated meaningfully with responses to other items on political economy.

TABLE 1. Support for State Role in the Economy by Political Status

Position	Mass Sample	Village Leaders	Township Leaders
+10	1.8%	3.6%	5.0%
+9	1.9	.0	2.5
+8	3.5	5.4	2.5
+7	5.7	5.4	5.0
+6	8.7	11.0	2.5
+5	12.0	16.0	15.0
+4	10.0	16.0	23.0
+3	9.4	11.0	10.0
+2	14.0	14.0	15.0
+1	7.5	5.4	7.5
0	15.0	7.1	13.0
−1	3.9	3.6	.0
−2	2.4	1.8	.0
−3	1.9	.0	.0
−4	.8	.0	.0
−5	1.3	.0	.0
−6	.3	.0	.0
−7	.1	.0	.0
−8	.0	.0	.0
−9	.0	.0	.0
−10	.1	.0	.0
Total	100.3%	100.3%	101.0%
Number of cases	721	56	40

Note: Positions are constructed by subtracting reported position on the importance of individual enterprises for economic development from reported position on the importance of state enterprises for economic development. Percentages do not add to 100 due to rounding.

Positions on the dimension are, indeed, associated with responses to many such items. I found that respondents who assigned relatively greater importance to state enterprises in economic development also reported significantly more negative assessments of the opportunities provided by the open market, lower appraisals of the usefulness of private enterprises, less tolerance of large income inequalities, and colder feelings toward private retailers and households that have prospered greatly under the reforms.[3] These results are consistent with a somewhat broader interpretation of positions on the relative importance of state and individual enterprises—as positions that approximate the direction and

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[1] In addition to evaluating the importance of state and individual enterprises, respondents were asked to evaluate the importance of collective enterprises for economic development. There is no unambiguous interpretation of those responses in a broader ideological sense, however. Most rural collective enterprises are managed by township governments or village administrations and combine administrative management and local initiative, thus sharing features of state and individual enterprises. This intermediate status, which frustrates ideological interpretation of responses, is reflected in Pearson correlations of responses. Evaluations of state and collective enterprises are highly correlated (0.47 for masses, 0.51 for leaders), as are evaluations of collective and individual enterprises (0.47 for masses, 0.35 for leaders); respondents seem to distinguish clearly between state and individual enterprises, however, as reflected in much lower correlation coefficients (0.12 for masses, 0.14 for leaders).

[2] I chose to use one measure rather than some combination of these items to analyze congruence. The questions on economic reform are quite different from one another in content. An argument can be made for combining some items (those on income inequality, for example), but constructing a composite index that taps substantively different issues of political economy imposes on the data a questionable presumed coherence. Nor does the presumed coherence actually measure up well in reliability analysis (e.g., examining Cronbach's alpha) of a scale combining the different items. The feeling thermometer items do form one factor in a factor analysis of feeling thermometer items, but my interest is congruence on issues.

[3] Pearson correlations are all statistically significant ($p \leq .001$, two-tailed tests), relationships are in the predicted directions, and the magnitude of coefficients ranges from 0.12 to 0.21 (with more than 700 cases). Question items are reported in Appendix B.

TABLE 2. Regression Coefficients for Village Leaders' Position on the State's Role in the Economy

Independent Variables		
Mean position of township leaders	.52**	(.21)
Mean position of village masses	.64*	(.33)
Village leader's year of birth $(\cdot 10^{-1})$	.12	(.53)
Village leader's education[a]	.21	(.13)
Village leader's household income[b]	.14	(.14)
Village 1990 mean per-capita income $(\cdot 10^{-2})$	−.26	(.21)
Intercept	−21.55	(103.34)
R^2	.19	
Standard error of the regression	2.55	
Number of cases	56 villages	

Note: Entries are unstandardized OLS regression coefficients; standard errors of coefficients appear in parentheses. Positions on the role of the state in the economy are constructed by subtracting the position on the importance of individual enterprises for economic development from the position on the importance of state enterprises for economic development. Question items are reported in Appendix B; variable characteristics and correlation coefficients are reported in Appendix C.

[a] Years of formal schooling.

[b] An ordinal variable that categorizes reported household income for 1989 in 500-*yuan* and 1000-*yuan* brackets, from less than 500 *yuan* to 10,000 *yuan* and more.

*$p \leq .05$ (one-tailed test).

**$p \leq .01$ (one-tailed test).

strength of orientations toward the economic role of the state.

ESTIMATING CONGRUENCE: VILLAGE LEADERS, SELECTORATES, AND ELECTORATES

In this section I use multivariate regression analysis to inquire into the existence of statistically and substantively significant congruence between village leaders and their selectorates and electorates. The question here is straightforward: Do selectorates and electorates of village leaders matter to those leaders, as reflected in orientations toward the role of the state in the economy? In the Chinese village context of 1990, it is clearly conceivable that both, one, or neither matter—or that they have some influence on the views of village leaders but in a way not reflected in the measure employed here.

The dependent variable in the analysis is the position of the village leader on the state's role in the economy. The independent variables of main analytic interest are the mean position of ordinary villagers (aggregated by village) and the mean position of township leaders (aggregated by township) on that issue dimension.[4] The analysis estimates the degree to which positions of village electorates and township selectorates have an effect on the positions of village leaders. Achen (1978) introduced the notion of congruence as "responsiveness" to characterize such estimates. Considered in that perspective, coefficients for the independent variables described above are interpretable as amounts by which a village leader can be expected to change position as the selectorate or electorate changes position by one unit on the dimension, other things equal.

Village leaders in the sample share a number of basic characteristics. Overwhelmingly, they are male, Communist Party members, and grew up in the village. There is more variation on other potentially relevant variables, however, which I include as control variables in the analysis: age, education, and household income.

Responsiveness is consistent with several explanations of congruence, but at least one cause does not necessarily imply responsiveness: Leaders and villagers may position themselves similarly on the state's economic role because positions are shaped by local economic environment, which they share. To rule out that explanation requires controlling for shared economic environment by including in the model an independent variable (or variables) to reflect local economic circumstances. Lacking a measure that reflects the configuration of state and private enterprises in the local economy, which would be ideal given the model specified, I used village mean per capita income as a summary measure of economic environment.[5]

Results of the analysis are presented in Table 2. Tests of statistical significance for control variables are two-tailed, as no direction is predicted. For the variables that aggregate positions of villagers and township leaders, I use a one-tailed test. The predicted direction of the relationship is positive.

Control variables in the model are not statistically significant.[6] Turning to the two variables of analytic interest, the mean positions of township leaders and ordinary villagers are statistically significant in a direction predicted by congruence.[7] Significance of the former is not surprising, considering the interest and practical involvement of township selectorates in choosing village leaders. The significant effect of the position of villagers is a more interesting result: Peasants are influential, even though the electoral process takes place in a formally Leninist context.

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[4] I also specified a model using the village median position as the measure of central tendency. For the selectorate sample, the mean equals the median. Results are similar, but with a smaller coefficient for the village median position (0.39, $p = 0.08$, one-tailed test). The spatial theory of elections offers no guidance in the form of a prediction that village leaders in the sample are closer to the village median position than to the mean, or vice versa. In villages with some electoral competition, more than three candidates are nominated for committee offices, and at least three are elected. If villagers vote for candidates nearest them, winners can be predicted to be nearer to the village median position than losers. Leaders in the sample are draws of one from at least three winning candidates per village, however. Among winners, leaders in the sample can be the farthest from the village median. Depending on the distribution of positions of villagers, leaders in the sample can be nearer to the village mean position than to the median.

[5] I defined local economic environment at the village level, but the results of the analysis are substantially the same, with coefficients remaining stable, when township mean per capita income is substituted. I have measures of the village per-capita value of output from the private sector, but only for 40 villages. I analyzed that subset, and the measure of local economic environment was not statistically significant; other results were similar, but with a smaller coefficient for the village mean position (0.61, $p = 0.09$, one-tailed test).

[6] The Pearson correlation of age and education is 0.41, but neither is statistically significant when the other is excluded. The lack of statistical significance for control variables may reflect their smaller variance (e.g., relative to a mass sample) and not their inherent irrelevance. In an individual-level regression model of the mass sample, with sex, age,

In substantive terms, too, the findings indicate that selectorates and electorates matter to village leaders. The amount of responsiveness to electorates, for example, can be expressed in the following way. The mean positions of villagers range in value from −0.33 to 5.1, a range of 5.4 points; multiplying this by the coefficient for the village mean yields a product of 3.5. As the range in positions for village leaders is 13 points, village electorates can be considered to "move" leaders more than one-fourth of the range—a result that is striking in its substantive significance.

EXPLAINING CONGRUENCE: THE ELECTORAL CONNECTION

A rich theoretical and empirical literature suggests several explanations of the relationship between elections and congruence. The most obvious is inspired by a normative theoretical perspective that views representatives as delegates who act on instructions from constituents, transmitting constituency preferences (see Pitkin 1967). Competitive elections ought to produce congruence because voters can choose the candidates closest to them on some dimension. To the degree that village elections offer choices, then, voters can reject candidates with positions distant from their own in favor of candidates closer to them. The spatial theory of elections arrives at the same conclusion and suggests an additional reason to expect a relationship: Competition pushes candidates toward the constituency center to capture votes. In the two-party model of Downs (1957), competition takes the form of bunching at the median to maximize votes; in multiparty models, parties compete by dispersing along an ideological dimension (Cox 1987, 1990), but the competitor at the median is the predicted winner, in some sense of the term.[8] The absence of campaigns in the context studied here suggests that candidate movement to capture votes is an unlikely explanation for congruence between leaders and voters. In the small village communities, information about where candidates stand is more than likely known by villagers outside (and long before) the electoral process.

A different electoral connection is suggested by Verba and Nie (1972). The competitive electoral process may encourage better communication between potential elites and masses, producing clearer understanding and agreement on the relevant issues. The logic of the relationship here is not Downsian. Villagers choose candidates closest to them, but all candidates (winners and losers) are closer to villagers because of the electoral process. Elections move candidates and villagers closer together because they produce a dialogue that allows candidates to know better what ordinary villagers think.

Congruence in the Chinese village context can also emerge through mechanisms other than elections. One explanation (explored above) is shared environment. Another is informal influence by villagers, with threats of social ostracism, for example (Parish and Whyte 1978). Before decollectivization, although rural grassroots leaders were selected not elected, they were (as now) from the village community and identified closely with it, siding with villagers against the encroachment of the state and other communities (Burns 1988, Shue 1988). Such prereform linkages of villagers and their leaders—whether through shared local environment, socialization, or informal influence—have nothing at all to do with a presumed electoral connection, and there is no reason to suppose that village elections preclude such linkages.

In this section I use multivariate regression analysis to examine intervillage variation in congruence. First, I address a basic question about the effect of democratization: Is congruence between village leaders and villagers to some significant degree the product of an electoral connection? The uneven progress of democratization in the countryside supplies the basis for an answer. If democratization is an important source of congruence, then leaders in villages that are farther along in implementing the law on village committees will be closer to their village constituents in their positions on the state's role in the economy than leaders in villages that lag behind in implementation, other things equal. Second, I attempt to sort out the causal mechanism underlying the electoral connection.

Dependent Variable: Distance between Village Leader and Village Electorate

The dependent variable used in this section measures the distance between positions of village leaders and positions of villagers on the economic role of the state. While positions of village leaders (the dependent variable above) on the dimension can be expected to depend on a variety of economic and other variables which also may affect the positions of villagers, in theory there is no reason to expect a systematic relationship between those variables and distance on the dimension.

As Achen (1978) notes, a straightforward measure of congruence, such as proximity, which he estimates as the average squared distance between a leader and constituents on some dimension, takes no account of the shape of the distribution of positions of constituents. Such a

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education, and household income as independent variables and position on the state's economic role as the dependent variable, age, sex, and income are all statistically significant. When age is taken out of the model, education is statistically significant.

[7] I examined Cook's D statistics to identify influential cases, using the heuristic suggested by Bowerman and O'Connell (1990, 468). In this analysis and others presented below, no cases were identified as influential.

[8] Under plurality electoral rules whereby only one competitor can win, the predicted winner is the one nearest the median; under proportional representation, if no single party is likely to win a majority of seats, then the median party can be expected to dominate government formation (Laver and Schofield 1990, 89–143).

measure can take on a large value, indicating great distance, because constituents are sharply divided between extremes on the dimension. By contrast, the measure can take on a small value, indicating close proximity, in a very homogeneous constituency because of a narrower distribution of positions on the dimension. A simple stylized comparison illustrates the problem. In two voting districts of two voters each, with voters in one district positioned four intervals apart on some dimension of interest and voters in the other district positioned on the very same point on the dimension, a leader who is positioned exactly between voters in the former district scores lower in congruence than does a leader positioned one interval away from voters in the latter district—even though the former leader better reflects constituency positions (indeed, reflects those positions as well as possible, given their distribution). To analyze congruence between leaders and their constituents, given some distribution of positions in the constituency, requires a measure that takes the variance of the distribution into account. Achen subtracts variance from average squared distance to create such a measure.

The dependent variable in the analyses below is that latter measure of congruence. To estimate it for each village, I computed the difference in positions between the village leader and each respondent in the village sample and then squared each of those differences. For each village I then computed the mean of those squared distances. Villages with similar average distances between the leader and individual villagers can be very different in terms of how those villagers are distributed on the dimension reflecting orientation toward the state's role in the economy. For example, villages 313 and 343 in the sample have mean squared distances of 12 and 12.6, respectively, but village 313 is much more homogeneous in its views—with a variance in mass position on the dimension of only 2.5, compared to 9.2 in village 343. To control for the contextual difference, the measure of ideological distance subtracts village variance in positions on the dimension from the village mean squared distance: For village 313 the resulting measure is 9.5, while for village 343 it is 3.4, for example. The smaller the value of the distance measure, the closer village leaders are to villagers in general; the larger the value, the more distant.

Independent Variables of Analytic Interest

Independent variables that test hypotheses about the effect of rural grassroots democratization on congruence between village leaders and villagers fall into three categories: institutional, attitudinal, and behavioral.

Electoral Competitiveness and Electoral Quality. Two institutional variables measure the quality of elections in the 56 villages. The most obvious, important, and easily measured index of progress in democratization is electoral competitiveness—the number of candidates on the ballot relative to the number of offices. Among the 56 villages surveyed, data on the relevant village committee elections are available for only 26 (spread across 9 of the 20 townships, in two counties).[9] In order to avoid dropping observations from an already small sample, I adopted an assumption about a relatively less democratized quality of elections in the 30 villages for which electoral data are unavailable, based on the following reasoning. Data on village elections are collected at the township level by the department of civil affairs. The fact that village electoral data are unavailable suggests that the townships (or counties) in which those villages are located did not attach a high priority to implementing the draft law on village committees in 1990, when the survey was conducted. For those 30 villages, I assumed the quality of elections reflected less progress in democratization compared to the villages for which electoral data are available, and I created a dichotomous variable (one if electoral data are available, zero otherwise) reflecting the assumed difference in electoral quality. The predicted relationship between the institutional variables and the distance between the village leader and village electorate is negative. Other things equal, the more competitive and democratized the election, the smaller is the distance between the leader and villagers.

Interest in Village Elections. A key attitudinal variable related to democratization is mass interest in village elections. Up to the mid-1980s, Chinese peasants generally ignored elections as a means of influencing public affairs. They voted, but this was a political ritual to which they attached little practical significance (Burns 1988). Interest in the elections has obvious implications for congruence between village leaders and villagers; insofar as the latter persist in viewing elections as largely irrelevant channels of influence, that view can be self-fulfilling. Villagers who see the elections as useful channels for affecting village affairs are more likely to influence candidate selection, for example. Votes of completely uninterested villagers are similar to random draws of candidates, producing a relationship between positions of voters and winners that will be similarly random. Thus, the predicted relationship between mass interest in village elections and distance between the village leader and the village electorate is negative. Other things equal, the distance between leaders and villagers in villages with more interest in elections can be expected to be smaller than in villages where there is less interest.

Voter Turnout and Attendance at Meetings. Behavioral indicators of participation also can reflect interest in village elections. Voter turnout may indicate mass interest in elections, since villagers who remain uninterested are unlikely to vote. Another behavioral indicator is attendance at village meetings. Villagers who are interested in local affairs and who consider formal channels of influence useful are likely to attend village meetings, which include study sessions to learn about villagers' rights and village committee functions, informal group deliberations in which villagers propose and discuss potential candidates, and all-village meetings in which villagers nominate candidates and hear them defend their qualifications for leadership.

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[9] Lack of availability refers to the fact that the data do not exist at the township level for the elections analyzed here, not that we were unable to obtain the data.

As indicators of interest in village elections, however, such behavioral measures may be problematic. While villagers in an Anhui community nominated 83 candidates and held five voting sessions to elect their village committee, for example, in some localities villagers participated only after mobilization efforts by election organizers (Zhang 1992). Before the recent changes in political organization, local elections and meetings had a strong mobilizational character (Burns 1988). Voter turnout and attendance at village meetings may still better reflect the ability of election organizers to mobilize villagers than the level of mass interest in elections and village affairs. Chinese accounts indicate that high turnout and high levels of participation in candidate nomination are viewed as key indicators of success in grassroots democratization. Because they are easily quantifiable, they are also useful criteria at higher political levels to evaluate township performance in organizing elections. This can transform them into performance targets. If voter turnout and attendance at meetings are products of mobilization, they can be unrelated to interest in elections and to congruence between leaders and villagers. Indeed, such mobilization can reflect an authoritarian approach to elections. If so, higher turnout and attendance at village meetings can be correlated with more, not less, distance between leaders and villagers.

For the behavioral variables, then, there are competing hypotheses about the direction of their predicted relationship with distance between the village leader and the electorate, depending on whether the measures reflect voluntary or mobilized participation.

Control Variables

I included in the analyses two kinds of control variables: village demographic characteristics and variables relating to village leaders and their incentive structure.

Village Demographics. The demographic variables are village size, mean village income per capita, and mean level of education in the village. Of the three, size appears to have clear predictable implications for congruence between leaders and villagers because of its relationship to informed voting. Chinese accounts indicate that it is more difficult to get villagers to participate in meetings and other activities associated with elections when villages are large or the population widely dispersed (see Qi 1990, Zhang 1992, Zhen and Shi 1990). As a result, villagers in those communities are less knowledgeable about their rights, village committee functions, and qualifications of specific candidates for leadership.

Village Leaders and Their Incentive Structure. In the sample, leaders include village committee directors and village Communist Party branch secretaries who serve concurrently as village committee deputy directors or ordinary members. While all members of village committees must be elected, party branch secretaries owe their party position mainly to higher level party organizations. Those who serve on village committees are elected to that position by villagers, but their party position does not depend on the quality of their relations with villagers. Elected or not, party branch secretaries are village leaders, and that difference may have implications for congruence. Those who owe their position of leadership only to the village electorate may better represent villagers.

A second potentially relevant dimension on which village leaders vary is experience. From one perspective, they have both incentive and opportunity to learn where villagers stand on issues that affect their daily lives. Leaders with more years of experience in village leadership may better represent prevailing issue orientations of villagers simply because they know villagers and their positions better. In 1990, however, village leaders with more experience were (by definition) those who had held power before the introduction of village elections. To the degree that more experience connotes success in meeting standards applied by selectorates before democratization, more experienced leaders may be more secure in their position and represent villagers less well.

Results

Results of the analyses are presented as Model 1 in Table 3. Where a direction of effect is predicted, the test of statistical significance is one-tailed; otherwise, the test is two-tailed. Two sets of results stand out. The first has to do with the electoral connection. Electoral competitiveness—the most obvious, unambiguous, and reliably measured index of grassroots democratization—is statistically significant in the predicted direction in both samples. While not statistically significant, the direction of the electoral quality coefficient in the 56-village sample is at least suggestive.[10] Context, however different, can only make so much difference; in Chinese villages, as in other contexts, voter choice and representation go together. Notwithstanding the interest and capacity of township selectorates to influence which candidates appear on final ballots, villagers choose the winners. The more opportunity villagers have to reject some candidates in favor of others, the nearer winners are to the position of villagers who choose them.

A second set of results points to the relevance of context and has to do with political participation: Behavioral variables are significantly associated with less congruence. In the 26-village sample, mass attendance at village meetings is associated with greater distance between leaders and villagers, and the direction of the voter turnout variable is suggestive (although not statistically significant); in the 56-village sample voter turnout is associated with greater distance, and the direction of the attendance at meetings variable is suggestive.[11] A

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[10] For electoral quality, $p \leq .10$ (one-tailed test). Given some fairly strong correlations between independent variables (which can present a multicolinearity problem) and the small number of observations, the result is worth noting. In the 26-village sample, $p \leq .10$ (one-tailed test) for mass interest in village elections, too; distance between leaders and villagers is smaller in villages with greater interest in the elections.

[11] In the 26-village sample, $p \leq .10$ (one-tailed test) for the voter

TABLE 3. Regression Coefficients for Distance between Village Leader and Village Electorate on the State's Role in the Economy

	Model 1				Model 2			
Independent Variables	26 villages		56 villages		26 villages		56 villages	
Electoral quality[a]	—		−28.02	(20.55)	—		−25.37	(18.80)
Electoral competitiveness[b]	−64.62*	(33.21)	−42.47*	(20.75)	−76.85**	(30.45)	−54.06**	(19.33)
Mass interest in village elections[c]	−22.55	(13.99)	−3.73	(6.33)	−20.36	(12.65)	−6.00	(5.83)
Voter turnout in village[d]	.60	(.36)	.53*	(.25)	.66*	(.32)	.57**	(.23)
Mass attendance at village meetings[e] $(\cdot 10^{-1})$	4.69*	(1.80)	1.37	(.88)	2.18	(1.99)	.62	(.84)
Functional position of village leader[f]	−5.55	(8.17)	−1.02	(3.67)	−6.64	(7.38)	−1.25	(3.36)
Village leadership experience[g] $(\cdot 10^{-2})$	−18.71	(33.14)	.58	(16.39)	10.84	(32.83)	−1.73	(15.01)
Village 1990 population (log)	3.21	(6.87)	−.31	(3.32)	.55	(6.31)	.76	(3.05)
Village 1990 mean per-capita income $(\cdot 10^{-3})$	−5.45	(14.13)	−6.99	(8.87)	10.93	(14.80)	−.89	(8.34)
Mass level of education[h]	−1.16	(4.23)	.48	(1.93)	.83	(3.92)	1.43	(1.79)
Distance on local problems[i]	—		—		20.55*	(9.48)	11.24**	(3.58)
Intercept	2.52	(58.31)	12.65	(26.64)	−27.32	(54.32)	−11.94	(25.59)
R^2	.39		.20		.53		.34	
Standard error of the regression	15.75		11.76		14.19		10.75	

Note: Entries are unstandardized OLS regression coefficients; standard errors appear in parentheses. Positions on the economic role of the state are constructed by subtracting the position on the importance of individual enterprises for economic development from position on the importance of state enterprises for economic development. Distance is mean squared distance minus village mass variance in positions. Question items are reported in Appendix B; variable characteristics and correlation coefficients are reported in Appendix C.

[a] Coded one if electoral data are available, zero otherwise.

[b] Excess candidates divided by number of positions (coded zero if data are unavailable).

[c] Village mean self-reported interest in village elections, four-level ordinal variable at individual level, ranging from "not interested" (low) to "very interested" (high).

[d] Coded zero if data are unavailable; otherwise, percentage of eligible voters who voted.

[e] Percentage of respondents in village who reported attending a village meeting recently.

[f] Coded one if village committee director only, zero if also Communist Party branch secretary.

[g] Number of years in any village leadership position.

[h] Village mean years of formal schooling for mass respondents.

[i] Mean difference between leader and village respondent evaluation of 13 local problems, four-level ordinal variable at individual level, ranging from "not a problem" (high) to "a very serious problem" (low).

*$p \leq .05$ (one-tailed test).

**$p \leq .01$ (one-tailed test).

reasonable interpretation is that in 1990, as in the past, easily quantifiable measures such as attendance at meetings and voter turnout reflect the ability of authorities to mobilize the masses to participate in not altogether voluntary activities. Certainly, O'Brien (1994) implies that representative village leadership is by no means necessarily associated with successful execution of unpopular state assignments; other things equal, village leaders who are better at mobilizing the masses may be poorer at representing them.

The analyses in Model 1 indicate that congruence is in significant part the result of connection involving democratized or competitive village elections, but they do not address the causal mechanism underlying the electoral connection. One perspective, suggested by Verba and Nie (1972), might consider electoral quality and competitiveness as measures of attention paid to the law on village committees. In villages where implementation of that law is taken more seriously, the electoral process presents an opportunity for a dialogue between leaders (including potential leaders) and ordinary villagers. While not incompatible with an argument that voters choose candidates closest to them in orientation, an enhanced understanding between elites and masses, not voter choice, may be the key causal mechanism underlying the electoral connection. All candidates (winners and losers) may be closer to villagers because of a process of communication in villages where the elections are given more attention.

The analyses in Model 2 are an initial attempt to sort out the mechanism underlying the electoral connection, by controlling for variation in diffuse understanding between leaders and villagers that may result from the electoral process as élite-mass dialogue. The variable added to the regression equation is based on responses of village leaders and villagers to a series of the same questions about the existence and seriousness of a variety of problems in the community: day-to-day supply of goods, medical care, income distribution, education, prices, public order, birth control, supply of energy resources, transport and communications, protection of the environment, equality of the sexes, industrial development, and agricultural development. The measure is computed at the village level: The average seriousness of each problem reported by villagers is subtracted from the village leader's evaluation of the seriousness of the

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turnout variable; in the 56-village sample, $p \leq .10$ (one-tailed test) for the attendance at meetings variable.

same problem; then, the average of the distance between leader and villagers across all problems is computed. The resulting variable is the distance between the village leader's understanding of the degree to which a number of issues are problems in the community and the mean understanding of villagers.

Results of the analyses suggest that the electoral connection is not simply explained by a view of elections as opportunities for elite-mass dialogue. In both samples there is significantly greater distance in villages where leaders and villagers do not share a close understanding of local problems, but the effect of electoral competitiveness does not go away; it remains significant in the predicted direction, and the effect of electoral quality remains suggestive.[12] Further research, focusing on process, is needed to elucidate the underlying causal mechanisms at work. Without data on all candidates, to demonstrate that winners are in fact closer to villagers than losers, the argument that congruence comes about because voters with more choice can choose candidates closer to them remains untested here. But the theoretical logic is well established, and the analyses demonstrate that congruence is in significant part the result of an electoral connection that involves voter choice per se: the more choice, the more congruence.

CONCLUSION

The changes that prompted this study are not nearly as radical as the democratic revolutions that swept away much of the communist world in 1989 and 1990. Nonetheless, the electoral connection in the Chinese countryside is also a revolution of sorts. In theory, it reverses the Leninist relationship between leaders and masses; in practice, it is transforming the relationship between leaders and ordinary citizens at the rural grassroots. As expected, village leaders are responsive to their selectorates above them at the township level, but they are also responsive to the villagers who elect them. Congruence between village leaders and villagers is in significant part the result of an electoral connection that involves voter choice.

The demand for rural grassroots democratization came in the late 1980s from the top not the bottom of the communist system. Chinese leaders hoped that popularly elected village committees would fill a vacuum in leadership created by agricultural decollectivization—and restore stability and enhance compliance in rural areas. Officially, the village committees are an experiment. Nonetheless, the democracy that is slowly growing in Chinese villages is likely to have implications as profound as the changes in economic organization that created the demand for it. Most provinces have already held two or three rounds of village elections. The "mundane revolution" of voter choice appears to be catching on in the countryside of one of the world's few remaining communist powers.

APPENDIX A. SAMPLING

The survey was conducted in four counties, one each in Anhui, Hunan, Hebei, and Tianjin. The 20 townships are a stratified probability-proportionate-to-size sample of five townships representing different population and income strata within each county. Villages are a probability-proportionate-to-size sample of three (administrative) villages within each selected township. All of the villages surveyed had undergone a round of elections after mid-1988, when the law on village committees officially took effect. The analyses in this article employ responses of villagers and their elected leaders in 56 of the villages surveyed, as well as responses of leaders at the higher township level. Four villages were omitted from the analysis because interviewers were unable to locate a village leader for interviewing in one of the villages, and three village leaders failed to respond to one of the items used to construct positions on the economic role of the state.

The mass sample, considered here as a sample of electorates for the village committees, consists of separate simple random samples (i.e., one per village) of resident adult villagers. Among 1,096 completed mass interviews in the 56 villages, 34% of respondents reported no position on one or both items used to construct positions on the economic role of the state, yielding 721 valid observations for analysis in the mass sample. Nearly half of all cases dropped because of missing data are respondents with no formal schooling. Nonetheless, the mass sample used for the analyses includes 119 respondents with no schooling—17% of the sample, compared to 28% of mass respondents with whom interviews were completed and who have no schooling.

The 56 elected village leaders surveyed are a nonprobability sample; 19 are directors of their village committee, and 37 are village Communist Party branch secretaries and serve concurrently as deputy directors or ordinary members of the village committee. The 40 township leaders surveyed are considered here as a sample of selectorates for leaders in villages under their respective jurisdictions. They are a purposive sample, consisting of a Communist Party secretary (or deputy secretary) and a township government head (or deputy head) in each of the 20 townships surveyed. As there are 20 selectorates for the 56 village leaders, each is a selectorate for 4 or 5 of the village leaders. That is, while electorates are unique to village leaders in the sample, leaders in villages located in the same township have the same selectorate.

APPENDIX B. MEASUREMENT

The following survey questions were used in the analyses. Numbering refers to items in elite and mass questionnaires, respectively (or the mass questionnaire, where only mass responses were used).

C4, E3. Here is a scale. The "0" on the scale indicates no importance and the "10" indicates extreme importance. How important would you say state enterprises are for economic development? How important would you say individual [i.e., private] enterprises are for economic development?

A4, A1. In what year were you born?

H1, A4. How many years of schooling have you had?

H20, G20. Please look at this card and choose a range that includes your family's total income for last year. Here under income we include salary, subsidies, pensions, payments in kind (such as grain, et cetera): under 500, 500–999, 1000–1499, 1500–1999, 2000–2499, 2500–2999, 3000–3499, 3500–3999, 4000–4999, 5000–5999, 6000–6999, 7000–7999, 8000–8999, 9000–9999, 10,000 and over.

C15. How interested are you in the elections for the village committee? Would you say you are very interested, somewhat interested, not too interested, or not interested?

C10. Have you attended an all-village meeting recently?

Sections E and F, C and F. Now I am going to present you with a number of views. We would like you to give your opinion on each viewpoint, by stating whether you strongly agree, agree, disagree, or strongly disagree: C1a, E4a. For the vast majority, the open market has not provided opportunities to get rich. C1d, E4d. It is better not to start up individual enterprises, because there is a chance they will fail. F2d, F5b. Income ceilings should be set to prevent very large income inequalities.

F4h, F4b. Here is a card with a thermometer drawn on it. If you have good feelings about a particular kind of person, you will place them at a point between 50 and 100 degrees, according to the strength of your feelings. And if you do not feel good about a particular kind of person, you will place them at a point between 1 and 50 degrees. If you have neither good nor bad feelings about a particular kind of person, you should choose 50 degrees. Now think about how you feel toward [read item]. At what point on this thermometer would you place them?: private retailers, 10,000-*yuan* households.B2, B11. Do you think any of the following are problems here? [If no, continue to next item. If yes, continue on same item.] Do you think this is a very serious problem here, serious, or not serious?: day-to-day supply of goods, medical care, income distribution, education, prices, public order, birth control, supply of energy resources, transport and communications, protection of the environment, equality of the sexes, industrial development, agricultural development.

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[12] For electoral quality, $p \leq .10$ (one-tailed test).

APPENDIX C. VARIABLE CHARACTERISTICS AND CORRELATION COEFFICIENTS

TABLE C.1. Variable Characteristics for Analysis Presented in Table 2

Variables	Mean	Standard Deviation	Minimum	Maximum
Village leader's position	3.84	2.67	−2.00	10.00
Mean position of township leaders	3.82	1.97	1.00	9.00
Mean position of village masses	2.85	1.11	−.33	5.07
Village leader's year of birth	1947.54	7.56	1924.00	1963.00
Village leader's education	7.11	2.93	.00	13.00
Village leader's household income	8.86	2.84	2.00	15.00
Village 1990 mean per-capita income	652.45	208.23	277.00	1469.00

Number of cases: 56 villages

TABLE C.2. Variable Characteristics for Analyses Presented in Table 3

Variables	Mean	Standard Deviation	Minimum	Maximum
Distance between leader and electorate	10.06	16.07	−1.14	66.90
Electoral competitiveness	.30	.18	.00	.67
Mass interest in village elections	2.55	.41	1.79	3.44
Voter turnout in village	87.51	10.91	58.81	96.67
Mass attendance at village meetings	50.04	33.91	.00	94.10
Functional position of village leader	—	—	.00	1.00
Village leadership experience	16.77	10.80	1.00	38.00
Village 1990 population (log)	7.02	.54	5.99	8.06
Village 1990 mean per-capita income	748.04	236.74	346.00	1469.00
Mass level of education	4.45	1.12	2.42	6.47
Distance on local problems	1.39	.58	.51	2.65
Number of cases: 26 villages				
Distance between leader and electorate	7.17	11.86	−1.14	66.90
Electoral quality	—	—	.00	1.00
Electoral competitiveness	.14	.19	.00	.67
Mass interest in village elections	2.32	.42	1.53	3.44
Voter turnout in village	40.63	44.65	.00	96.67
Mass attendance at village meetings	39.93	29.69	.00	94.10
Functional position of village leader	—	—	.00	1.00
Village leadership experience	17.16	10.05	1.00	38.00
Village 1990 population (log)	7.16	.51	5.99	8.12
Village 1990 mean per-capita income	652.45	208.23	277.00	1469.00
Mass level of education	4.42	1.01	2.11	6.50
Distance on local problems	1.45	.52	.51	2.65

Number of cases: 56 villages

TABLE C.3. Pearson Correlation Coefficients for Variables in Analysis Presented in Table 2

Variables	1	2	3	4	5	6	7
1	1.00						
2	.22	1.00					
3	.21	−.19	1.00				
4	.02	−.09	−.19	1.00			
5	.15	−.16	.00	.40	1.00		
6	.11	.28	−.02	−.30	−.21	1.00	
7	.02	.53	−.14	−.14	.01	.40	1.00

Number of cases: 56 villages

Key to variables:

1. Village leader's position
2. Mean position of township leaders
3. Mean position of village masses
4. Village leader's year of birth
5. Village leader's education
6. Village leader's household income
7. Village 1990 mean per-capita income

TABLE C.4. Pearson Correlation Coefficients for Variables in 26-village Analyses Presented in Table 3

Variables	1	2	3	4	5	6	7	8	9	10	11
1	1.00										
2	−.13	1.00									
3	−.03	.45	1.00								
4	.19	.41	.04	1.00							
5	.17	.63	.77	.06	1.00						
6	.02	−.41	−.47	−.15	−.34	1.00					
7	−.00	−.21	−.31	−.09	−.17	.23	1.00				
8	.01	−.13	−.11	−.02	−.14	.35	.31	1.00			
9	−.16	−.09	−.20	−.10	−.24	−.03	−.04	.05	1.00		
10	−.18	.62	.45	−.01	.55	−.19	−.05	.13	−.05	1.00	
11	.37	.51	.58	.11	.75	−.20	−.33	−.10	−.47	.32	1.00

Key to variables:

1. Distance between village leader and village electorate
2. Electoral competitiveness
3. Mass interest in village elections
4. Voter turnout in village
5. Mass attendance at village meeting
6. Functional position of village leader
7. Village leadership experience
8. Village 1990 population (log)
9. Village 1990 mean per-capita income
10. Mass level of education
11. Distance on local problems

TABLE C.5. Pearson Correlation Coefficents for Variables in 56-Village Analyses Presented in Table 3

Variables	1	2	3	4	5	6	7	8	9	10	11	12
1	1.00											
2	.23	1.00										
3	.10	.78	1.00									
4	.11	.51	.58	1.00								
5	.26	.99	.81	.51	1.00							
6	.14	.32	.55	.73	.32	1.00						
7	.04	.09	−.11	−.21	.07	−.19	1.00					
8	.02	−.04	−.13	−.15	−.05	−.06	.11	1.00				
9	−.08	−.26	−.26	−.11	−.26	−.15	.05	.10	1.00			
10	−.01	.43	.29	.23	.41	.11	−.06	.08	−.01	1.00		
11	−.07	.03	.31	.13	.03	.22	.04	−.06	.01	−.18	1.00	
12	.31	−.10	.16	.32	−.09	.49	−.17	−.05	−.12	−.22	.08	1.00

Key to variables:

1. Distance between village leader and village electorate
2. Electoral quality
3. Electoral competitiveness
4. Mass interest in village elections
5. Voter turnout in village
6. Mass attendance at village meetings
7. Functional position of village leader
8. Village leadership experience
9. Village 1990 population (log)
10. Village 1990 mean per-capita income
11. Mass level of education
12. Distance on local problems

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Uncertainty, Shifting Power, and Appeasement

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Great Britain faced an immensely complicated strategic problem in the 1930s, and important aspects of it can be stylized as a situation in which a state that is declining in power is unsure of the aims of a rising state. If those aims are limited, then the declining state prefers to appease the rising state's demands rather than go to war to oppose them. If, however, the rising state's demands are unlimited, then the declining state prefers fighting. And, given that the declining state is becoming weaker over time, it prefers fighting sooner rather than later if there is to be a war. This situation creates a trade-off: The earlier a state stands firm, the stronger it will be if war ensues, but the higher the chance of fighting an unnecessary war. In equilibrium, the declining state generally tries to appease the rising state by making a series of concessions.

During the 1930s Great Britain tried to appease Germany by acceding to its demands to revise the international status quo in its favor. Had German demands been limited, appeasement might very well have averted an unnecessary war. As it turned out, those ambitions were not limited, and Britain and France declared war on a Germany that was much stronger in 1939 than it had been in the early 1930s.[1]

Although "appeasement" is often identified with British foreign policy during the 1930s and especially with Munich and the Czech crisis of September 1938, the problem of appeasement is much broader both historically and theoretically. Kennedy (1983), for example, traces the history of Britain's policy of appeasement back to the 1860s. More theoretically, several important aspects of the strategic problem facing Britain in the 1930s can be generalized and stylized as a situation in which a state that is declining in power is unsure of the aims of a rising state. If those aims are limited, then the declining state prefers to appease the rising state's demands rather than go to war to oppose them. If, however, the rising state's demands are unlimited, then the declining state prefers fighting. And, given that the declining state is becoming weaker over time, it prefers fighting sooner rather than later if there is to be a war. The problem confronting the declining state in this situation is to decide just how much to concede and when to draw the line and fight if necessary. The dilemma facing the rising state is to decide how much to demand and when to stop making further demands.[2]

Two fundamentally important issues in international relations theory lie at the heart of this stylization. First, how do shifts in the distribution of power affect states' calculations and behavior? Second, how do states cope with uncertainty about the extent of other states' aims and ambitions?

Despite the importance of the problem of appeasement and of these two issues more generally, few if any formal models in international relations theory incorporate both a shifting distribution of power and uncertainty about the scope of another state's ambitions. Models in which the distribution of power may change over time have generally assumed that the players have complete information about each other. That is, each state knows the other's payoffs and the extent of its ambitions (e.g., Fearon 1993, 1996; Kim and Morrow 1992). Conversely, models in which a state is uncertain about the scope of another state's ambitions have generally taken the distribution of power to be fixed (e.g. Bueno de Mesquita and Lalman 1992; Fearon 1995; Morrow 1989; Powell 1996, forthcoming).

The goal of this study is to examine the combined effects of a shifting distribution of power and of uncertainty about the extent of an adversary's ambitions by analyzing these issues in the simplest possible formal setting. Focusing on a simple setting brings both a substantive and a methodological advantage. Substantively, this focus centers attention on the fundamental trade-offs that define the strategic problem facing the states and the ways in which the states deal with these trade-offs. These basic trade-offs would be present in more complicated models. Methodologically, understanding the dynamics of appeasement in a simple setting may make it clearer how to generalize and extend the basic model in the most productive ways.

The simplest setting turns out to be that of a "simple timing game" (Fudenberg and Tirole 1991, 117–28). In a simple timing game, each player has to decide at each moment whether to continue or to stop. The game ends as soon as one of the players stops. In the model analyzed here, the declining state must decide whether to continue the game by granting another concession to the rising state or to end the game by standing firm and going to war if necessary. The rising state must decide at each moment whether to continue by making another

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[1] The literature on appeasement and British foreign and defense policy during the 1930s is, of course, vast. Important contributions include: Barnett (1972), Bond (1980), Boyce and Robertson (1989), Dilks (1981), Gilbert (1966), Howard (1972), Kennedy (1976, 1978, 1992), Middlemas (1972), Mommsen and Kettenacker (1983), Murray (1984), Parker (1993), Peden (1979), Post (1993), A. J. P. Taylor (1962), and T. Taylor (1979).

[2] There were other important aspects of the strategic problem facing Britain, such as its relations with France, Italy, and Japan; whether Britain standing firm would be perceived by Germany as a threat, thus contributing to a spiral of hostility; or whether British concessions would only whet Germany appetite for further concessions.

demand or to stop the game by deciding not to make any further demands.

The declining state is also unsure of the rising state's ambitions in this game. If those ambitions are limited, then the declining state would rather satisfy those demands than fight in order to oppose them. This willingness to satisfy limited demands makes the declining state vulnerable to "salami" tactics (Schelling 1966, 66–9). That is, as long as the declining state believes that the rising state will only make a few more demands before stopping, the declining state would rather accede to those demands than bear the cost of fighting. If, however, the rising state's ambitions are unlimited, then the declining state prefers fighting. And because the declining state is becoming weaker over time, it is better to fight sooner rather than later if there is to be a fight.

The combination of a shifting distribution of power and uncertainty about the rising state's ambitions creates an inescapable trade-off. The earlier the declining state draws the line, the stronger it is, and the higher its expected payoff in the event of war. But the earlier it draws the line, the higher the probability of an unnecessary war. That is, the earlier the declining state stands firm, the greater the chances that it will fight a rising state whose demands are sufficiently limited that the declining state would have preferred satisfying them to fighting *had the declining state known the extent of those demands*.

In equilibrium, the declining state must balance these two opposing factors and generally does so by adopting a strategy or policy of appeasement. It makes a series of concessions in the hope of satisfying the rising state. Eventually, however, the declining state draws a line and is unwilling to make any further concessions. If these concessions have satisfied the rising state, then the confrontation ends peacefully. If the rising state is still dissatisfied, then war results. In pursuing this policy, the declining state necessarily accepts some risk that there will eventually be a fight and that it will occur on less favorable terms. This is the unavoidable price that the declining state must pay for the chance of averting an unnecessary war.

Two versions of the simple timing game are analyzed. In the first, the declining state becomes weaker and weaker as time passes. There are infinitely many equilibria in this case of continual decline. Indeed, the declining state's decision to stand firm at almost any juncture can be rationalized or justified as the outcome of some equilibrium. This multiplicity of equilibria exists because the credibility of the declining state's threat to stand firm depends on its expectations about what would happen if it did not stand firm. If it expects the rising state to make so many more demands that the payoff to fighting now is at least as large as the payoff to granting those additional concessions, then the declining state can credibly threaten to stand firm, and the game will end. In the case of continual decline, however, there is nothing to fix or anchor these expectations about how much more the rising state will demand. Thus, for almost any moment, there exists an associated set of expectations which would rationalize stopping at that time. In the second version of the game, the declining state's deteriorating position is expected to bottom out eventually and then improve somewhat. This prospect effectively serves as an expectational anchor. This case of eventual improvement generally has a unique equilibrium.

The aspects of appeasement stylized and analyzed here are closely related to power-transition theory (Organski 1968, Organski and Kugler 1980, Kugler and Lemke 1996). A central element in each is how a declining state copes with the demands of a rising state. Indeed, the game analyzed below may also be interpreted as a model of a power transition. Power-transition theory argues that a more rapid decline makes war more likely (Organski and Kugler 1980, 21). A comparative-statics analysis of the equilibria derived below give this claim some formal support. A more rapid transition does make war more likely in the model.

This paper is organized as follows. The next section presents the simple timing game, discusses some of its limitations, and relates it to other work. The subsequent section characterizes the equilibria of the complete-information game. There follows an analysis of the incomplete-information versions of the game. A final section examines the effects of the speed of the power shift on the probability of war.

A MODEL OF APPEASEMENT

Appeasement is a response to a strategic problem. One state decides to make concessions to another as a way of dealing with the strategic situation confronting it. Many different strategic environments may lead to a policy of appeasement, however. The present analysis focuses on a situation characterized by three features and models this setting in the simplest possible way. The disadvantage of a simple model is that it necessarily leaves out or minimizes other aspects of the problem. The advantage here is that it brings the interaction of the three factors to the fore, helps to clarify that interaction, and points the way to other questions and more elaborate models which can incorporate other aspects.

The first feature is a satisfied state content with the international status quo and a dissatisfied state seeking to revise the status quo in its favor. The second is that the satisfied state has incomplete information about the extent of the dissatisfied state's ambitions. If these are sufficiently limited, then the satisfied state prefers to accede to them rather than fight. This, for example, generally reflects the British attitude toward Germany's desire to overturn many of the restrictions placed on it by the Versailles Treaty (Gilbert 1966). If, however, the dissatisfied state's demands are sufficiently large, then the satisfied state prefers fighting to meeting those demands. The third feature is the dynamic nature of the strategic problem facing the satisfied state. Its power is declining, that is, its expected payoff to using force to oppose the dissatisfied state is falling over time. If, therefore, the satisfied state can be certain it eventually will fight the dissatisfied state, then it prefers to fight sooner rather than later. The assumption that the satisfied state's position is deteriorating makes it costly simply to wait to see if the dissatisfied state's ends are limited. As it waits, the declining state becomes weaker and weaker, and its prospects for prevailing in any conflict become smaller and smaller.

These three features can be formalized in a simple timing game between a declining state, D, and a rising state, R. Each player must decide at each instant whether to continue or to stop, and the game ends when one of the players stops. In the present model, R must decide at each instant whether to make another demand or to accept the status quo and stop making demands. D, in turn, must decide whether to make a further concession or to draw the line and fight if necessary.

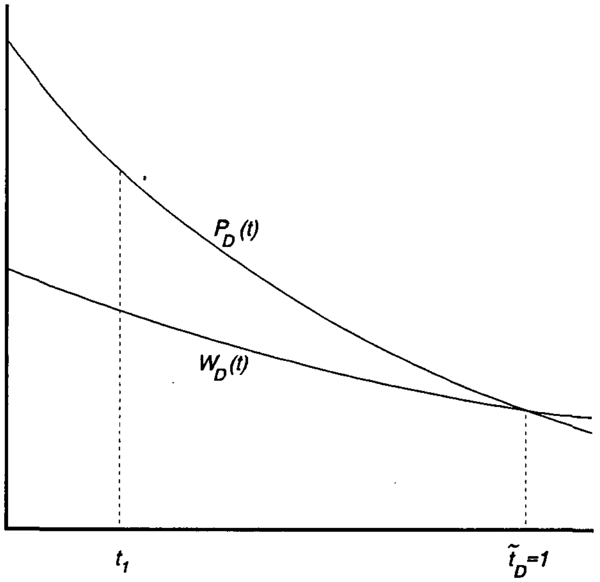
The game ends in war if D stops first. The substantive interpretation here is that D's stopping is really a decision to go to war unless R stops making demands. If, therefore, R continues when D stands firm, D will go to war in the face of further demands from R. (As will be seen, actually carrying out its threat to go to war in the face of further demands is in D's own self-interest in equilibrium.) If R stops first, the game ends peacefully.[3] The idea is that D will only go to war if it believes it will pay too high a price in order to satisfy R's future demands. Once R stops making demands, D has no reason to fight.[4]

If the game ends peacefully at time t, D's payoff is $P_D(t)$. If the game ends in war, D's payoff is $W_D(t)$. Figure 1a illustrates D's payoffs. Both $P_D(t)$ and $W_D(t)$ are decreasing in t. $P_D(t)$ decreases because D is making concessions throughout the game. Thus, the longer the game lasts, the more D will concede to R and the lower D's payoff from a peaceful resolution. D's payoff if the game ends in war, $W_D(t)$, falls because the declining state is becoming weaker and so will do less well in the event of war.

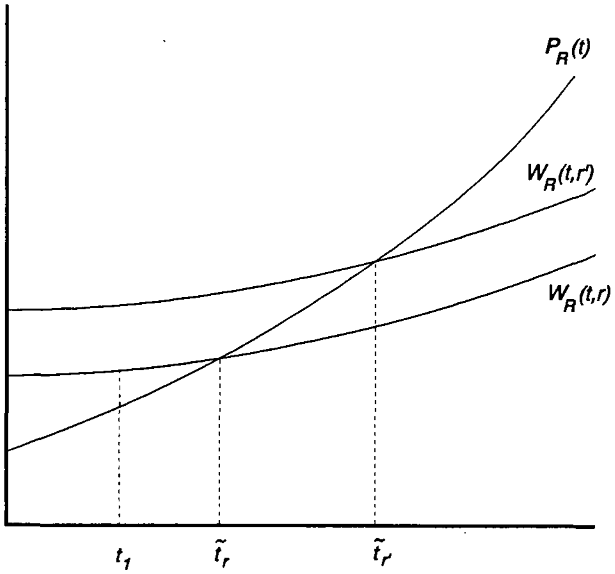
Furthermore, P_D starts out above W_D, with the distance between them becoming smaller and smaller. This assumption formalizes the idea that the declining state is willing to satisfy the rising state's demands if these are sufficiently limited. If, for example, D is sure that R will stop after D accedes to a series of demands that take it to t_1, then D prefers appeasing R to fighting at time zero. In symbols, $P_D(t_1) > W_D(0)$. Indeed, the declining state is always vulnerable to salami tactics as long as $P_D(t) > W_D(t)$. That is, there is always a concession sufficiently small that D prefers granting it to fighting if D is certain this will be the last demand.[5]

The payoff functions, $P_D(t)$ and $W_D(t)$, also are assumed to cross at $\tilde{t}_D$, that is, $P_D(\tilde{t}_D) = W_D(\tilde{t}_D)$. The motivation for this assumption is that there is a point, $\tilde{t}_D$, at which the declining state is unwilling to make any further concessions regardless of how much more it expects R to demand. After $\tilde{t}_D$, D's payoff to fighting is strictly greater than its payoff to a peaceful settlement. If the game lasts this long and D finds itself at a time later than $\tilde{t}_D$, then it will end the game. For convenience, the payoffs are assumed to have been normalized so that $\tilde{t}_D = 1$.[6]

FIGURE 1. The Payoff Functions of Declining and Rising States



(a) The Declining State's Payoffs



(b) The Rising State's Payoffs

The crucial aspect of the rising state that the model is designed to capture is that R strictly prefers war to peace as long as its minimal demands are not satisfied. Once these are satisfied, further concessions make R better off. More is better than less. But R will not go to war to secure additional concessions. Figure 1b illustrates these ideas. If R stops first, thereby ending the game peacefully, then it obtains $P_R(t)$: $P_R(t)$ increases because the longer the game lasts, the more D concedes to R, and the better off R will be.

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[3] By assumption, the game ends peacefully if D and R stop simultaneously. This assumption will be discussed more fully below.

[4] There is a simplification here. Games of timing end when one of the players stops. One consequence of this is that once R decides not to make an additional demand, the game ends, and R cannot decide to start making demands again at a later time. More general formulations would allow for this possibility.

[5] More formally, if $P_D(t) > W_D(t)$, then continuity ensures there is a Δt sufficiently small that $P_D(t + \Delta t) > W_D(t)$.

[6] As discussed more fully below, the assumption that $P_D(t)$ and $W_D(t)$ cross is not crucial and can easily be relaxed.

The rising state's payoff if the confrontation ends in war at time t is $W_D(t, r)$. The parameter r denotes the rising state's willingness to go to war. The larger is r, the larger $W_D(t, r)$; that is, $W_D(t, r') > W_D(t, r)$ whenever $r' > r$. Most narrowly, one can think of r as the inverse of the rising state's cost of fighting. The lower the cost, the higher is r, and the larger the rising state's payoff to the gamble of fighting. Alternatively, one can think of r as inversely related to the rising state's level of risk aversion. The more risk averse the rising state, the lower is r, and the lower its expected payoff to fighting. Most broadly, r reflects the rising state's level of resolve. The more resolute the rising state, the higher is r, and the higher its expected payoff to fighting. $W_D(t, r)$ also increases as t does, because the rising state is becoming more powerful, and consequently the expected payoff in the event of war increases over time.

$P_R(t)$ crosses $W_R(t, r)$ at time $\bar{t}_r$, that is, $P_R(\bar{t}_r) = W_R(\bar{t}_r, r)$. This is the time at which the rising state's minimal demands have just been satisfied. R at this time is indifferent between fighting or accepting the status quo. If the declining state stands firm earlier than $\bar{t}_r$, for example, at t_1 in Figure 1b, then the rising state prefers to fight rather than accept the current distribution of benefits. Formally, $W_R(t, r) > P_R(t)$ for $t < \bar{t}_r$. If the declining state stands firm after $\bar{t}_r$, the rising state is satisfied. It still benefits from additional concessions (P_R is rising). But if faced with the stark choice of accepting the current status quo or going to war to change it, the rising state chooses peace since $P_R(t) > W_R(t)$ for $t > \bar{t}_r$. Finally, note that the greater R's resolve, the more it takes to satisfy r's minimal demands, and thus the larger is $\bar{t}_r$, that is, $\bar{t}_r$ is increasing in r.

The third and final feature of the model is the declining state's uncertainty about the rising state's ambitions. This feature will be formalized more fully below. Less formally, D is unsure of how much it will take to satisfy R's minimal demands, that is, where r's breakeven time, $\bar{t}_r$, is located. At one extreme, R's demands may be sufficiently limited that D prefers appeasing those demands to fighting. That is, D's payoff to satisfying R's minimal demands is greater than D's payoff to fighting even in the best circumstances, which is at the very outset before its power begins to decline. In symbols, $P_D(\bar{t}_r) > W_D(0)$. At the other extreme, R's demands may be so large that D prefers to fight.

To simplify the analysis, the following discussion focuses on the case in which the states' payoff functions are linear. The Appendix shows how these results can be extended to the more general payoff functions described above. To specify the linear case, let $P_D(t') = q' - c't'$ and $W_D(t') = w' - d't'$, where q', c', w', d' are all positive; $q' > w'$; and $c' > d'$ because P_D is steeper than W_D. One can think of c' as the rate at which D makes concessions and of d' as the rate at which D's power is declining. Normalizing P_D and W_D and rescaling time gives $P_D(t) = 1 - ct$ and $W_D(t) = 1/2 - dt$, where $c - d = 1/2$.[7] D, moreover, is indifferent as between war and peace at $t = 1$. Turning to R's payoff, P_R and W_R will be normalized so that $P_R(t) = pt$ and $W_R(t, r) = r + wt$. The time at which the rising state's minimal demands are satisfied is given by $P_R(\bar{t}_r) = W_R(\bar{t}_r, r)$ or $\bar{t}_r = r/(p - w)$, where $p > w$ by assumption.

An example makes these linear payoffs more concrete. Suppose that the territorial status quo initially favors D, and R is considering making a series of territorial demands. To simplify matters, assume that the amount of territory that can be divided ranges between zero and one and that the cumulative amount of territory R will have obtained at time t is just t. For example, at the outset, when $t = 0$, R has yet to demand anything, and the cumulative concessions that D has made are zero. Later, at time $t = 1/2$, R's cumulative demands amount to half the territory. If the game ends peacefully at this point, then the territory will be evenly divided between D and R. If the game lasts until $t = 1$, then R will obtain all the territory.

If the game ends peacefully at time t, the amount of territory D and R have is $1 - t$ and t, respectively. Assuming that the states value territory directly, perhaps because of the resources it provides, D's and R's payoffs if the game ends peacefully at t are $P_D(t) = 1 - t$ and $P_R(t) = t$. As these functions indicate, the value of controlling all the territory is assumed to be one for both of the states, while the value of not controlling any territory is zero.

To specify the payoffs if the confrontation ends in war, suppose that territory contributes not only to a state's utility but also to the resource base it needs to fight. The more territory a state controls, the higher is the probability that it will prevail. Thus, the more the declining state concedes, the weaker it becomes, and the stronger the rising state becomes. Most simply, assume the probability that the challenger will prevail if it controls an amount of territory t is t. Suppose further that when a state prevails it gains control of all the territory. War is costly, however, and the value of that territory is $1 - k$, where k reflects the cost of fighting. If defeated, then a state loses control of all the territory, and its payoff is zero. These simplifications imply that D's expected payoff to fighting at t is the payoff to prevailing multiplied by the probability of prevailing plus the payoff to losing multiplied by the probability of losing. In symbols, D's payoff is $W_D(t) = (1 - k)(1 - t) + 0 \cdot t = (1 - k) - (1 - k)t$, and R's payoff is $W_R(t) = (1 - k)t + 0 \cdot (1 - t) = (1 - k)t$. This example provides one possible interpretation of the linear payoffs described above.

Before analyzing the game's equilibria, it is useful to end this section by briefly discussing two of the model's limitations and by relating the model to other work on power transitions and to other formal analyses in international relations theory that use games of timing. The most important limitation is inherent in the formulation of the model as a simple timing game. In order to focus on the timing of a significant decision, which in this context is the rising state's decision to stop making further demands and the declining state's decision to draw the line, the players' set of options in simple timing games is extremely limited. A player can either continue or stop. The players, for example, cannot choose among different possible ways of continuing. Put another way, there is really no bargaining in a simple timing game; the size of the offers and demands are exogenous. To wit, the rising state cannot decide how much to demand at any one time. It also cannot decide, for example, to wait without making any demands until it becomes more powerful and then makes a large demand, which is the problem Kim and Morrow (1992) study. Analogously, the declining state can either accede to the current demand or draw the line. It cannot, for example, demonstrate that it is willing to accept some demands but not others, as the British cabinet ultimately showed by mobilizing the fleet after Hitler raised his demands at the second meeting with Chamberlain at Godesberg (Parker 1993, 168–70; T. Taylor 1979, 819–27, 881–9). Nor can the declining state decide how much it is willing to concede to the rising state and then make that offer, which is the problem Fearon (1996) analyzes in a complete-information context. Simple timing games abstract away from these complexities by only letting the players decide whether to continue, not how to continue.

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[7] To conduct this normalization and rescaling, let $t = (c' - d')/(q' - w')t'$, multiply P_D and W_D by $1/(2(c' - d'))$, and then add $1 - q'/(2(q' - w'))$. This leaves $c = c'/(2(c' - d'))$ and $d = d'/(2(c' - d'))$.

A second limitation is that there is only one-sided incomplete information. Although the declining state is uncertain of the rising state's willingness to fight, the latter is completely certain of the declining state's resolve. Technically, the assumption of one-sided incomplete information makes it much easier to characterize the game's equilibria. Substantively, this assumption tends to focus the analysis primarily on the strategic problem confronting the declining state and only secondarily on the rising state.

Indeed, the present formulation really only examines at most half the problem a declining state may face. In addition to being uncertain of the rising state's minimal demands, the declining state may also fear that if it stands firm for defensive reasons, even after satisfying the rising state's minimal demands, this action may be interpreted as a threat. The rising state may then go to war, not because it is fundamentally dissatisfied but because it feels threatened. Members of the British cabinet, for example, argued at various times during the 1930s that taking a hard line would not be seen in German eyes as a defensive response but as a threatening move. The present model is too spare to capture this dynamic. (See Jervis 1977, 62–78, for a discussion of the spiral process and Kydd 1993 for an effort to model it.)

Games of timing are widely used in economics (see Fudenberg and Tirole 1991, 117–28, for an overview) and increasingly so in international relations theory (e.g., Fearon 1994; Kim and Morrow 1992; Nalebuff 1986; Powell 1987, 1988, 1989, 1990). Two general types of timing games are a war of attrition and a preemption game. In a war of attrition, two actors are competing for a prize. The longer the competition lasts, the higher is the price each actor pays. In the game, each actor decides how long it will fight for the prize, and the struggle continues until one of the actors stops. The prize then goes to the player willing to fight the longest. The distinguishing features of wars of attrition are that each player prefers the other to stop first, and the sooner it stops the better (Fudenberg and Tirole 1991, 122). These features characterize many situations, including patent races in which each firm must decide when to stop investing in R&D (Fudenberg et al. 1983) and exit decisions in which a firm must decide when to shut down and leave a declining industry (Fudenberg and Tirole 1986, Ghemawat and Nalebuff 1985). Wars of attrition have also been used to model crises (Fearon 1994; Nalebuff 1986; Powell 1987, 1988, 1989, 1990). In these games, each state wants to prevail, but escalation is costly, and the longer the crisis lasts the more costly it becomes. The central decision facing a state in these models is to decide when to stop escalating and end the crisis.

Preemption games are roughly the opposite of wars of attrition. In the latter, each player wants the other to stop first. In the former, each player wants to preempt the other by being the first to stop and end the game.[8] For example, each of two firms may be trying to decide when to enter a market. The first to enter captures the market and reaps monopoly profits. If both enter at the same time, they must divide the market, and both will be unprofitable (Fudenberg and Tirole 1986). As a second example, two states may be mobilizing in a crisis in which, as in July 1914, first-strike advantages are believed to exist. These create an incentive for each state to try to preempt the other with a first strike.

An unusual feature of the model analyzed here is that it combines elements of both a war of attrition and a preemption game. As Figure 1a illustrates, the declining state's payoffs are typical of those in a war of attrition (at least for times less than one). The declining state prefers the rising state to stop first, and the sooner the better. More formally, $P_D(t) > W_D(t)$, and $P_D(t)$ is decreasing in t. In contrast, the rising state's payoffs are typical of those in a preemption game. Once its minimal demands are satisfied, the rising state prefers to stop first, thereby securing the payoff to a peaceful resolution. But the later it stops, the more it attains from the declining state and the higher its payoff. That is, $P_R(t) > W_R(t)$ after R's minimal demands are satisfied and $P_R(t)$ is increasing.

COMPLETE-INFORMATION EQUILIBRIA

Although incomplete information is a central feature of the strategic problem facing the declining state, it is useful to begin by examining the simpler problem in which the declining state is certain of the scope of the rising state's ambitions. The basic approach and intuition underlying the analysis of this simpler game carry over to the more complicated case in which there is incomplete information. As will be seen, the complete-

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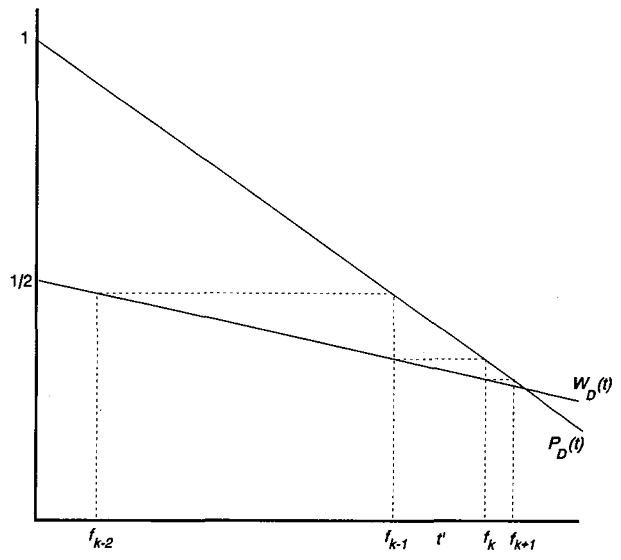
[8] Of course, there must be some incentive to wait. Otherwise, both players would stop at the very outset of the game. One incentive may be that it is very costly if both players stop simultaneously. Alternatively, it may be that the payoff to preempting increases over time.

FIGURE 2. The Complete-Information Stopping Times



information game has a multiplicity of equilibria, such that for any plausible stopping time, there exists a pure-strategy, subgame-perfect equilibrium in which the declining state draws the line and stands firm at that time.[9] The rising state anticipates that time in equilibrium and also stops making demands at this point. Thus, the game ends peacefully. Because the probability of war is zero in these equilibria, they are efficient in the sense that no payoffs are Pareto superior to the equilibrium payoffs.

In order to describe the equilibria and see why there are so many of them, consider D's decision at some time f_k in Figure 2. Assume for the moment that D can credibly commit itself to standing firm at f_k, and consider the consequences of tracing this commitment back up the tree. As long as R's minimal demands are satisfied, R will stop at f_k because D is standing firm there. The declining state therefore receives a payoff of $P_D(f_k)$.

Now consider D's decision at a somewhat earlier time, say, t'. Suppose that when deciding what to do at t', D expects R to continue until f_k and then stop. If D stands firm at t', then there will be war, and D will receive $W_D(t')$. If D waits until f_k to stand firm, then it will receive the payoff to settling peacefully at f_k, which is $P_D(f_k)$. D, therefore, prefers to wait until f_k to stand firm as long as $W_D(t') < P_D(f_k)$ or, equivalently, as long as $t' > f_{k-1}$, where f_{k-1} solves $W_D(f_{k-1}) = P_D(f_k)$. Put another way, the cost of granting the concessions from t' to f_k is less than the cost of fighting at t' as long as $t' > f_{k-1}$. D, therefore, is vulnerable to salami tactics in that it will not want to stand firm at t' if it expects R to make only a few more demands before quitting. Thus, D accedes to R's demands at all $f_{k-1} < t' < f_k$.

Now consider D's decision at f_{k-1}. Standing firm at f_{k-1} weakly dominates continuing. If D stands firm at f_{k-1}, then it will obtain at least $W_D(f_{k-1})$. If D continues, then it can do no better than continuing on to f_k, where it obtains $P_D(f_k)$. But $W_D(f_{k-1}) = P_D(f_k)$, so D always does at least as well by standing firm at f_k as it does by continuing. Thus, D can credibly commit to standing firm at f_{k-1} *given that it has credibly committed itself to standing firm at* f_k. Arguing as before, D's standing firm at f_{k-1} can rationalize a decision to stand firm at f_{k-2}, where $W_D(f_{k-2}) = P_D(f_{k-1})$. Working back up the tree generates a sequence of stopping times, $f_0, f_1, f_2, \ldots, f_{k-1}$, where D's standing firm at any f_j is rationalized by D's standing firm at f_{j+1}.

This sequence of stopping times rests on the assumption that D can credibly commit itself to standing firm at f_k. How can this commitment be rationalized? Paralleling the previous construction, D's firm stand at f_k can be rationalized by D's standing firm at f_{k+1} if the payoff to stopping at f_k is at least as large as the payoff to acceding to R's demands. In symbols, D's standing firm at f_k can be rationalized by the expectation that if D does not, then R will continue making demands until f_{k+1}, where $W_D(f_k) = P_D(f_{k+1})$. Continuing in this way, f_{k+1} can be rationalized by f_{k+2}, which satisfies $W_D(f_{k+1}) = P_D(f_{k+2})$. In turn, f_{k+2} can be justified by f_{k+3}, where $W_D(f_{k+2}) = P_D(f_{k+3})$, and so on, to generate a sequence of stopping times, $f_0, f_1, \ldots, f_k, \ldots$.[10]

This sequence forms the basis of an equilibrium. D's stopping at any f_j is rationalized by the expectation that if it does not stop and the game continues, then R will continue making demands until f_{j+1}. Thus, D's payoff to standing firm at f_j—even if that means fighting at f_j—is at least as large as its payoff to acceding to R's demands until f_{j+1}. Given these expectations, D can credibly commit to standing firm at f_j.

Unfortunately, this equilibrium is not unique. Indeed, there is a multiplicity of equilibria because D's standing firm at any particular time is based solely on its expectation about R's future demands, and there is nothing to anchor these expectations. So, for example, the declining state's decision to stand firm at any time t_0 can be justified by the expectation that if D does not stand firm, R will continue to make demands until t_1, where $W_D(t_0) = P_D(t_1)$. Similarly, D's standing firm at t_1 can be rationalized by the expectation that R will continue until t_2, where $W_D(t_1) = P_D(t_2)$, and so on. Since W_D and P_D are decreasing, the sequence $t_0, t_1, t_2, \ldots$ is sure to exist, and consequently there exists an equilibrium in which D initially stands firm at t_0.

The previous discussion suggests the following proposition, which is stated informally here and more formally in the Appendix.

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[9] The present analysis focuses throughout on pure-strategy equilibria. What it means for a stopping-time to be plausible will be defined below.

[10] As long as the payoff functions P_D and W_D are strictly decreasing, this sequence can always be constructed regardless of whether P_D and W_D cross, as is assumed here, or never intersect. If these functions do cross, then the sequence converges to the time at which the payoff functions intersect. If the payoffs do not intersect, then the f_k goes to infinity.

PROPOSITION 1: *Let* f_0 *be any plausible stopping time.*[11] *The following strategies then form a subgame-perfect equilibrium of the complete-information game: Both* D *and* R *stop at times* $f_0, f_1, f_2, \ldots$ *where* f_{k+1} *solves* $W_D(f_k) = P_D(f_{k+1})$. D *and* R *continue at all other times.*

Four properties of these equilibria should be emphasized. First, the declining state generally tries to appease the rising state by granting its demands until the declining state finally stands firm at f_0. Appeasement is an equilibrium solution to the strategic problem facing the declining state. Interestingly, the declining state may make these concessions even though it knows that the rising state's minimal demands have been satisfied and consequently, is, unwilling to fight to further its ends. As suggested above, the rising state may still gain further concessions because the declining state is always vulnerable to salami tactics. As long as the declining state believes that the rising state will only demand a little more, then the declining state too is unwilling to use force to oppose the rising state. This vulnerability gives the rising state an incentive to claim that its ends are limited even if they are not. This vulnerability also means that the declining state can credibly commit to standing firm and thereby compel the rising state to stop only if the declining state believes that continuing means granting so many additional concessions that the payoff to fighting now is at least as high as the payoff to acceding to this series of demands.

Second, the equilibria are efficient. The probability of war is zero, and the states successfully avoid the losses associated with fighting. The absence of war is due to the complete-information assumption. When the declining state is unsure of the rising state's minimal demands, there will generally be some risk of war in equilibrium, as will be shown in the next section.

Third, any efficient division of the resources saved by not fighting can be sustained in some equilibrium. To see this, note that D can assure itself of at least $W_D(0)$ in the game by standing firm at the very outset. By contrast, the best outcome D can obtain will occur when D stands firm as soon as R's minimal demands are satisfied and the game ends peacefully at that time. Doing so leaves D with $P_D(\tilde{t}_r)$. Thus, D's payoffs range from $W_D(0)$ to $P_D(\tilde{t}_r)$. But the range of plausible stopping times is just the set of times t, such that $W_D(0) \leq P_D(t)$ and $\tilde{t}_r \leq t$. Hence, for any payoff in the range from $W_D(0)$ to $P_D(\tilde{t}_r)$, there exist a stopping time and an associated equilibrium which yield that payoff to D.

Finally, the equilibria exhibit a bootstrap quality reminiscent of the punishment strategies in the subgame-perfect equilibria of infinitely repeated games. To wit, the declining state can credibly commit to standing firm at f_0 because both states expect it to stand firm at f_1. But these expectations are based on both states' expectations that the declining state will stand firm at f_2. These expectations are rationalized, in turn, by the expectation that the declining state will stand firm at f_3, and so on, for the infinite sequence of stopping times. These expectations are self-reinforcing, as they must be in an equilibrium, but they are also quite arbitrary. As noted above, this arbitrariness accounts for the multiplicity of equilibria.

Putting this point in another and more substantive way, the time and place at which the declining state stands firm depend on its expectations about R's future demands and have little or nothing to do with "the situation on the ground." An outside observer of the interaction between D and R could never explain why D stood firm at time t_1 rather than t_2 solely on the basis of the differences between the situations confronting D and R at t_1 and t_2. Since D always is willing to concede just a little more if it is sure R will then stop, D's stopping at one time but not another may appear quite arbitrary, inexplicable, or even irrational. The explanation lies not in the present situation but in expectations about the future.

INCOMPLETE INFORMATION AND CONTINUAL DECLINE

This section examines the case in which the declining state is uncertain of the rising state's minimal demands and in which the declining state's position or power deteriorates throughout the transition.[12] The equilibria in this case of continual decline closely resemble the complete-information equilibria. A multiplicity of equilibria remains. For any plausible stopping time f_0, there exists an equilibrium in which D stands firm at f_0. As in the complete-information case, D's decision to stand firm is rationalized by a series of future stopping times, $f_1, f_2, f_3, \ldots$. Because the declining state generally makes some concessions, that is, $f_0 > 0$, appeasement is still an equilibrium solution to the strategic problem confronting it. The rising state's equilibrium strategy is to stop at f_0 if its minimal demands are satisfied at that point and to continue if they are not. Thus, the probability that R will also stop when the D initially stands firm is the probability that R's minimal demands are satisfied at f_0. This probability will be less than one in general, which implies there is some chance that D's optimal policy of appeasement will end in war.

As before, the states are assumed to have linear payoff functions. D's are $P_D(t) = 1 - ct$ and $W_D(t) = 1/2 - dt$, where $c - d = 1/2$. R's payoffs are $P_R(t) = pt$ and $W_R(t) = r + wt$. D, moreover, is unsure of the rising state's resolve, r. In particular, D believes that r is uniformly distributed over $[\underline{r}, \bar{r}]$. Type r's minimal demands are just satisfied at $\tilde{t}_r$, where $\tilde{t}_r$ solves $W_R(\tilde{t}_r) = P_R(\tilde{t}_r)$ or, equivalently, $\tilde{t}_r = r/(p - w)$. Thus, the larger r, the tougher or more resolute is the rising state. The lower bound $\underline{r}$ is defined so that if the declining state is facing the least resolute type of rising state, namely, $\underline{r}$, then the declining state strictly prefers to satisfy $\underline{r}$'s minimal demands.[13] The upper bound $\bar{r}$ is defined so that the declining state will fight immediately if it ever concludes that it is facing $\bar{r}$.[14]

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[11] If D's initial stopping time is to be f_0, then it must be optimal for D to wait from the start of the game at $t = 0$ until f_0 before standing firm. Assuming that R's minimal demands will have been satisfied by f_0, that is, $f_0 \geq \tilde{t}_r$, then R will stop at f_0 if D stands firm there, which leaves D with a payoff of $P_D(f_0)$. Clearly, this payoff must be at least as large as D's payoff to fighting at the very outset, when it was strongest; otherwise, D will prefer to stand firm at $t = 0$. In symbols, f_0 must satisfy $W_D(0) \leq P_D(f_0)$. A stopping time f_0 will be said to be *plausible* if it satisfies $f_0 \geq \tilde{t}_{\underline{r}}$, and $W_D(0) \leq P_D(f_0)$.

[12] That is, $W_D(t)$ is strictly decreasing over [0, 1].

The intuition underlying the equilibrium in the complete-information case is that in order for D to stand firm credibly at f_0, D's payoff to fighting there must be just equal to its payoff to acceding to R's demands from f_0 to f_1 and then receiving the payoff to settling peacefully at f_1, which is $P_D(f_1)$. That is, the gain to settling peacefully at a later date is just offset by the cost of the future concessions D must make in order to secure that peaceful settlement. Similarly, D can commit itself to standing firm at f_1 because the gains to waiting until f_2 are just offset by the cost, and so on, for f_2, $f_3, \ldots$.

Extending this intuition to the incomplete-information case, suppose D's strategy is a series of stopping times $f_0, f_1, f_2, \ldots$. It is clear what R's strategy must be in this equilibrium. A type r's best response to D standing firm at $f_0, f_1, f_2, \ldots$ is to continue as long as its minimal demands are not satisfied. Once they are satisfied, r will certainly accept any further concessions D makes but will not fight to gain more and so stops when D next stands firm. More formally, r stops at the smallest f_k, such that r's minimal demands are satisfied at f_k.

Now consider D's decision at f_0. In equilibrium D's payoff to stopping must be at least as large as its payoff to continuing until the adjacent stopping time, f_1. Indeed, these two payoffs must be the same. To see this, note that if the payoff to stopping at f_0 is strictly larger than the payoff to waiting until f_1 to stop, then the payoff to stopping at a t slightly later than f_0 also is strictly larger than the payoff to waiting until f_1 to stand firm. But this implies that D will stand firm at t in a perfect equilibrium, which contradicts the assumption that f_0 and f_1 are adjacent stopping times. Accordingly, D's equilibrium payoff to standing firm at f_0 must equal its payoff to continuing until f_1.

Equating the expression for the declining state's payoff to standing firm at f_0 and f_1 gives[15]

$$W_D(f_0) = P_D(f_1)\,\frac{\Pr\{r : f_0 \le \bar{t}_r < f_1\}}{\Pr\{r : \bar{t}_r \ge f_0\}} + W_D(f_1)\,\frac{\Pr\{r : \bar{t}_r \ge f_1\}}{\Pr\{r : \bar{t}_r \ge f_0\}}. \qquad (1)$$

This equation has a simple interpretation. The expression on the second line is D's expected payoff to stopping at f_1, given that it is facing only those types of R whose minimal demands are not satisfied at f_0, namely, r for whom $\bar{t}_r \ge f_0$. Thus, f_1 is the stopping time for which D's expected value of continuing to f_1 is just equal to its payoff to fighting at f_0, and equation 1 itself is the incomplete-information analogue of the complete-information requirement that $W_D(f_0) = P_D(f_1)$.

Generalizing, f_{k+1} is the stopping time at which D's payoff to fighting at f_k equals its payoff to continuing until f_{k+1}, given that it is facing only those types of r whose minimal demands are not satisfied at f_k. In symbols,

$$W_D(f_k) = P_D(f_1)\,\frac{\Pr\{r : f_k \le \bar{t}_r < f_{k+1}\}}{\Pr\{r : \bar{t}_r \ge f_k\}} + W_D(f_1)\,\frac{\Pr\{r : \bar{t}_r \ge f_{k+1}\}}{\Pr\{r : \bar{t}_r \ge f_k\}}.$$

Solving for the sequence $f_0, f_1, f_2, \ldots$ in the case in which the payoff functions are linear and r is uniformly distributed over $[\underline{r}, \bar{r}]$ gives

$$\begin{aligned} f_1 &= 1 - 2d\bar{t}_{\bar{r}} + 2df_0 \\ f_2 &= 1 - 2d\bar{t}_{\bar{r}} + 2df_1 \\ &= 1 - 2d\bar{t}_{\bar{r}} + 2d(1 - 2d\bar{t}_{\bar{r}}) + (2d)^2 f_0 \end{aligned}$$

and

$$f_k = (1 - 2d\bar{t}_{\bar{r}}) \sum_{i=0}^{k-1} (2d)^i + (2d)^k f_0.$$

As long as $d < 1/2$, $\{f_k\}_{k=0}^{\infty}$ is well defined and converges to $f^* = (1 - 2d\bar{t}_{\bar{r}})/(1 - 2d)$.[16] Since $f^* < 1$, the sequence $\{f_k\}$ defines D's actions in the interval $[0, f^*)$.[17] D stops at all $f_0, f_1, f_2, \ldots$ and continues at all times in between. A type r stops at the first f_k at which its minimal demands are satisfied.

To complete the description of the equilibrium, the states' strategies in the interval $[f^*, 1]$ must be specified. Consider D's beliefs at the stopping times $f_0, f_1, \ldots$. Since each r stops at the first f_k at which its minimal demands are satisfied, D believes that if it continues to f_{k+1}, it will only be facing those r whose breakeven times are later than f_k. The later the stopping time f_k, therefore, the more confident D becomes that it is facing a type that it is unwilling to satisfy, namely, a type r for which $\tilde{t}_r > 1$. At some point, which formally turns out to be f^*, D becomes sufficiently confident that it is facing such a type that D is no longer willing to make any concessions. The now very slim chance that D will satisfy R by acceding to another demand and waiting a little longer to fight is not worth the cost of being that much weaker if there is a fight. Because D is unwilling to concede further, it stands firm, and so do all those r whose minimal demands are satisfied.

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[13] More formally, the rising state of type $\underline{r}$ strictly prefers war to peace at all times $t < \bar{t}_{\underline{r}}$. D, therefore, cannot possibly satisfy $\underline{r}$'s minimal demands unless D prefers the peaceful payoff obtained at $\bar{t}_{\underline{r}}$ to what can be obtained by fighting at the very outset. Consequently, $\bar{t}_{\underline{r}}$ must satisfy $P_D(\bar{t}_{\underline{r}}) \ge W_D(0)$.

[14] In symbols, $W_R(1, \bar{r}) > P_R(1)$, which leaves $1 < \bar{t}_{\bar{r}}$.

[15] D's payoff to standing firm at f_0 is the peaceful payoff to stopping there multiplied by the probability that R's demands are satisfied at f_0 plus the payoff to fighting multiplied by the probability that R's demands are not satisfied. In symbols, $P_D(f_0)\Pr\{r : \bar{t}_r < f_0\} + W_D(f_0)\Pr\{r : \bar{t}_r \ge f_0\}$. If, however, D deviates from its equilibrium strategy of stopping at f_0 and continues until f_1, then its payoff will be $P_D(f_0)\Pr\{r : \bar{t}_r < f_0\} + P_D(f_1)\Pr\{r : f_0 \le \bar{t}_r < f_1\} + W_D(f_1)\Pr\{r : \bar{t}_r \ge f_1\}$. The first term is just D's payoff from those r whose minimal demands are satisfied by f_0 and who therefore stop in equilibrium at f_0. (Since D's equilibrium strategy is to stand firm at f_0, the best reply for all those r whose minimal demands are satisfied at f_0 is also to stop.) The second term is D's payoff to settling peacefully at f_1 with those r whose minimal demands are satisfied between f_0 and f_1. The third term is D's payoff to fighting those r who are still dissatisfied at f_1. Equation 1 follows immediately.

[16] The assumption that f_0 is plausible implies that $d < 1/2$.

[17] f^* is less than one because $\bar{t}_{\bar{r}} > 1$ and $d < 1/2$. $\bar{t}_{\bar{r}}$ is greater than one because the toughest type, $\bar{r}$, is assumed not to break even before $t = 1$. Plausibility requires that D's payoff to standing firm at f_0 must be at least as large as its payoff to standing firm at $t = 0$. Algebra shows that these constraints imply that $d < 1/2$.

Summarizing this intuition more formally, the following strategies and beliefs form a perfect Bayesian equilibrium (PBE) in the interval $[f^*, 1]$: D stands firm at all times in this interval and believes at any time $t \in [f^*, 1]$ that it is facing only those types of r whose breakeven times, $\tilde{t}_r$, are at least t, that is, $\tilde{t}_r \geq t$. The strategy of r in this interval is to stop as soon as its minimal demands are satisfied, which occurs at $\tilde{t}_r$, since D is standing firm at all times.[18]

It is straightforward to verify that the following strategies and the beliefs derived from them by Bayes's rule form a PBE of the game: D stops at $f_0, f_1, f_2, \ldots$ and at all times in the interval $[f^*, 1]$ and continues at all other times. A type r continues until its minimal demands are satisfied and then stops whenever D stops. To see that these strategies do form a PBE, note that, by construction, D is indifferent between stopping at f_j and any other f_k. Again, by construction, if D is at any t between an f_i and f_{i+1}, D strictly prefers continuing until f_{i+1} to standing firm at t. Thus, D's stopping at each f_j and at all $t \geq f^*$ while continuing at all other times is a best reply at all times t to the rising state's strategy. Furthermore, r's strategy of continuing until its minimal demands are satisfied and then stopping if and only if D stops is a best reply to D's strategy.[19]

As in the complete-information case, there is a multiplicity of equilibria depending on the initial stop time f_0. Any plausible f_0 can serve as a seed for the sequence

$$f_k = (1 - 2d\tilde{t}_{\bar{r}}) \sum_{i=0}^{k-1} (2d)^i + (2d)^k f_0.$$

Thus, an equilibrium can be constructed around any plausible f_0.[20]

The assumption that the state's payoffs are linear and that r is uniformly distributed over $[\underline{r}, \bar{r}]$ can be generalized to allow for essentially any pair of payoff functions in which W_D is decreasing and for any distribution of beliefs. The approach to proving this more general result is to show that a sequence of stopping times f_0, f_1, $f_2, \ldots$ can still be constructed for any plausible stopping time f_0. The Appendix states the formal result and outlines the argument. Proposition 2 summarizes the results less formally:

PROPOSITION 2: *If* D*'s payoff to fighting,* $\mathrm{W_D}$*, is strictly decreasing and* D*'s payoff functions satisfy some minor technical requirements specified in the Appendix, then for any plausible stopping time* $\mathrm{f_0}$*, there exists a perfect Bayesian equilibrium in which the declining state stands firm at* $\mathrm{f_0}$*. Types of* R *whose minimal demands are satisfied at* $\mathrm{f_0}$*, that is, those types for which* $\tilde{\mathrm{t}}_\mathrm{r} \leq \mathrm{f_0}$*, continue until* $\mathrm{f_0}$*, where they stop making further demands. Other types do not stop and fight* D *at* $\mathrm{f_0}$.

In sum, the declining state generally makes some concessions in the hope of avoiding war. Appeasement remains an equilibrium solution to the strategic problem facing the declining state. Unlike the complete-information case, the equilibria are not efficient. When D lacks complete information about the extent of R's minimal demands, there is a positive probability that the strategy of appeasement will end in war. This positive probability of fighting on worse terms is the price the declining state must pay in order to avoid some unnecessary wars.

INCOMPLETE INFORMATION AND EVENTUAL IMPROVEMENT

The preceding analysis assumed that the declining state's position deteriorates continually throughout the period. Formally, W_D is strictly decreasing in t for $0 \leq t \leq 1$. This section examines the situation in which the declining state's position eventually bottoms out and then improves to some extent. In these circumstances, the declining state has two reasons to appease the other state. As before, the declining state may be able to avoid an unnecessary war if a few more concessions will satisfy the rising state's minimal demands. Now, however, appeasement may also be at least in part a policy for buying time. If the declining state fails to satisfy the rising state's minimal demands and eventually fights, then the declining state may be stronger and therefore in a better position to fight.

This case is motivated by both formal and historical considerations. Formally, the essence of the problem facing the declining state is whether it can credibly commit itself to standing firm. This credibility rests on the expectation that if D does not stand firm now, then R will make so many more demands that D would have been better off fighting at the outset rather than settling peacefully after satisfying those demands. In the case of continual decline, there is a multiplicity of equilibria because there is nothing to anchor these expectations. In the case of eventual improvement, by contrast, there is an expectational anchor. The fact that D's position eventually bottoms out provides an endpoint condition which generally pins down a single set of expectations and a unique equilibrium.

The historical debate about the value of buying time at Munich motivates the case of eventual improvement. As German pressure on Czechoslovakia mounted, some British officials, especially those representing the military, argued that Britain's relative military position would be better in a few months (Howard 1972, 123). In reviewing the situation on September 20, 1938, General Hastings Ismay, who was secretary to both the Committee on Imperial Defense and the Chiefs of Staff subcommittee, found "that, from a military point of view, time is in our favour . . . . If war with Germany has to come, it would be better to fight them in say six to twelve months' time, than to accept the present challenge" (quoted in T. Taylor 1979, 835). Just before the final Cabinet meeting to discuss the crisis, General Henry Pownall, Director of Military Operations and Intelligence, prepared an "appreciation" of the military situation (T. Taylor 1979, 886–7). He found that "from the military point of view the balance of advantage is definitely in favor of postponement. This is probably an exception to the rule that 'no war is inevitable,' for it will almost surely come later" (quoted in T. Taylor 1979, 887). Indeed, Chamberlain, in trying to convince the Cabinet to accede to Hitler's Godesberg demands, argued that Britain would be in a better position to fight later (Parker 1993, 168–70; T. Taylor 819–27). He asked the Cabinet to "consider whether the protection which we could afford today to the people of this country against German bombs was as effective as the protection which we might be able to offer in the future." After the fact, as British forces were trying to escape the beaches of Dunkirk, Chamberlain wrote privately: "It is clear as daylight that, if we had had to fight in 1938, the result would have been far worse" (quoted in T. Taylor 1979, 837, 985).

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[18] These strategies are discussed more fully in the Appendix.

[19] It is also easy to verify that D's beliefs satisfy Bayes's rule.

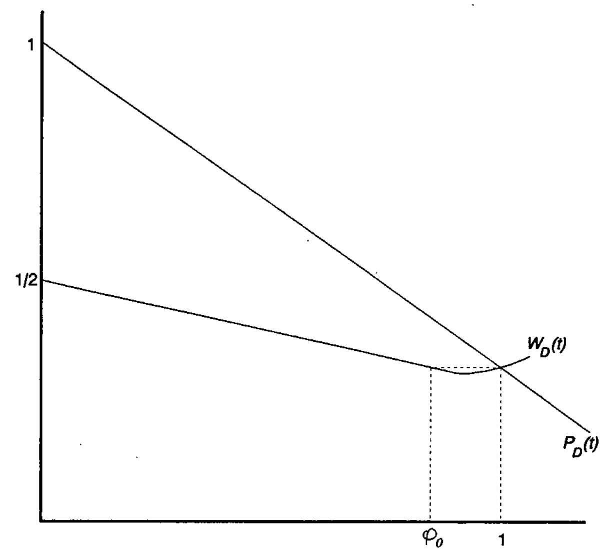
[20] f_0 is plausible as long as the payoff to standing firm initially at f_0 is at least as large as the payoff to standing firm at the outset.

Based on this evidence, some historians have explained Britain's behavior as an effort to delay war until British defenses, especially its air defenses, were stronger (see T. Taylor 1979, 984, and the citations in Murray 1979, footnote 2). Other historians have challenged this explanation on at least two fronts. The first is that a careful analysis of the politico-military situation at the time would have shown that Britain and France in combination with the Czech forces would have been stronger compared to Germany in 1938 than in 1939, when Britain stood firm against Germany over Poland (Murray 1984; T. Taylor 1979, 984–9). It also seems clear that while Chamberlain may have argued, and perhaps believed, that Britain would be stronger in future, the possibility of buying time had little effect on his thinking. He was confident that Germany could still be appeased and would have tried to do so regardless of any improvement in Britain's strategic position (Aster 1989, 249–50; Parker 1993, 168–9).

The crucial point for the formal analysis is not the relative weight the declining state attaches to the prospect of appeasing the rising state or to buying time. Rather, the point is whether or not the declining state's position declines continually or will eventually improve. If the former, then the model analyzed above is more appropriate; if the latter, then the model considered in this section is more suitable. To illustrate the point, suppose Chamberlain did believe that just one more concession would have satisfied Hitler. Chamberlain then would have been willing to make that concession whether or not he also believed that Britain would be stronger later. If, however, he also happened to believe that Britain would be stronger later, then the eventual-improvement model is a better formulation.

To formalize the case of eventual improvement, suppose that D's payoff to fighting, $W_D(t)$, decreases until it reaches a minimum and then increases until $t = 1$, where $W_D(t)$ crosses $P_D(t)$, and the game ends. Take ϕ_0 to be the time at which the declining state's payoff to fighting equals its payoff at $t = 1$, that is, ϕ_0 solves $W_D(\phi_0) = W_D(1) = P_D(1)$. As illustrated in Figure 3, ϕ_0 is earlier than the time at which $W_D(t)$ reaches its minimum; therefore, $W_D(t)$ is decreasing from $t = 0$ through ϕ_0.

FIGURE 3. The Declining State's Payoffs with Eventual Improvement



As in the case of continual decline, it is useful to focus on the situation in which D's payoffs are linear over the interval $[0, \phi_0]$, and r is uniformly distributed over $[\underline{r}, \bar{r}]$. Assume, therefore, that $P_D(t) = 1 - ct$ for $t \in [0, 1]$, and $W_D(t) = 1/2 = dt$ for $0 \leq t \leq \phi_0$. (As will be seen, the precise shape of $W_D(t)$ over $[\phi_0, 1]$ is unimportant.) Two features characterize the unique equilibrium. First, the declining state will never stand firm between ϕ_0 and 1 but does stand firm at ϕ_0 (and of course at $t = 1$). Second, D's standing firm at ϕ_0 then pins down a sequence of stopping times, $\phi_0, \phi_1, \phi_2, \ldots$, which forms the basis of the unique equilibrium.

To see that D continues over the interval $(\phi_0, 1)$ and stands firm at ϕ_0, suppose an equilibrium in which D stands firm at a $t' \in (\phi_0, 1)$. Now consider D's payoff for waiting until $t = 1$ to stand firm. Since D stands firm at t', all types of r with breakeven times of t' or earlier also will stop. Consequently, D only faces those r with breakeven times later than t'. By waiting until $t = 1$ to stop, D may induce some of these types to settle peacefully. D will benefit from this because the payoff to settling peacefully between t' and $t = 1$ is at least as large as D's payoff to fighting at t'.[21] If D happens to be facing one of those types who will not stop before $t = 1$, then D still benefits from postponing a fight from t' to $t = 1$ because D will be stronger at $t = 1$ than at t'. Thus, D unambiguously benefits by waiting at t', but this contradicts the assumption that stopping at t' is part of an equilibrium strategy.

Given that D will continue between ϕ_0 and $t = 1$, D can credibly commit to standing firm at ϕ_0 and does so in equilibrium. This is shown formally in the Appendix. The intuition is that D can credibly commit because its payoff to standing firm at ϕ_0 is at least as large as its payoff to continuing at ϕ_0. To see this, note that if D stands firm at ϕ_0 it can do no worse than receive its payoff to fighting at ϕ_0, which is $W_D(\phi_0)$. If D continues at ϕ_0, then, as just shown, it also will continue until $t = 1$, where it will obtain $P_D(1)$ if it settles peacefully and $W_D(1)$ if it fights. But $W_D(1) = P_D(1) = W_D(\phi_0)$, which implies that D's payoff to standing firm at ϕ_0 is at least as large as its payoff to continuing.

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[21] That is, $P_D(t) > W_D(t')$ for any t between t' and 1, because $P_D(t) \geq P_D(1) = W_D(1) > W_D(t')$.

The fact that D stands firm at ϕ_0 pins down a unique sequence of stopping times leading up to ϕ_0. Paralleling the analysis of the continuous-decline equilibrium, standing firm at ϕ_0 means that D can credibly commit to a firm stand at the earlier time ϕ_1, at which point D's payoff to fighting is equal to that of waiting until ϕ_0 to stand firm. That is, ϕ_1 satisfies equation 1, which leaves:

$$W_D(\phi_1) = P_D(\phi_0)\,\frac{\Pr\{r : \phi_1 \le \bar{t}_r < \phi_0\}}{\Pr\{r : \bar{t}_r \ge \phi_1\}} + W_D(\phi_0)\,\frac{\Pr\{r : \bar{t}_r \ge \phi_0\}}{\Pr\,\{r : \bar{t}_r \ge \phi_1\}}.$$

With linear payoffs and a uniform distribution over the type of R, the previous expression reduces to $\phi_1 = \bar{t}_{\bar{r}} - (1 - \phi_0)/(2d)$. Continuing in this way generates a sequence $\{\phi_k\}$, where $\phi_{k+1} = \bar{t}_{\bar{r}} - (1 - \phi_k)/(2d)$. This sequence is strictly decreasing and unbounded.[22] Accordingly, there exists a smallest integer, N, such that ϕ_N is the first plausible stopping time. The sequence ϕ_0, $\phi_1, \ldots, \phi_N$ forms the basis of a unique equilibrium of the game, which is summarized here and stated more formally in the Appendix.

PROPOSITION 3: *If there is eventual improvement, then there is a unique perfect Bayesian equilibrium. In it,* D *stands firm at* $\phi_0, \ldots, \phi_N$ *and* $t = 1$ *and continues at all other times. A rising state of type* r *continues at all* $t < \bar{t}_r$. *If* $t > \bar{t}_r$, *then* r *stops whenever* D *stands firm and continues whenever* D *continues.*

In sum, there is a unique equilibrium when the declining state's position eventually reaches a low point and then at least partially recovers. Appeasement remains an equilibrium solution to the strategic problem facing the declining state. There is, moreover, a positive probability that the optimal policy of appeasement will end in a war fought on worse terms. As in the case of continual deterioration, this positive probability of war is the price the declining state must pay in order to have some chance of avoiding unnecessary wars.

COMPARATIVE STATICS AND THE SPEED OF THE TRANSITION

Organski and Kugler (1980, 21) argue that the faster a power transition occurs, the more likely it is to end in war.[23] The present model offers some formal support for this claim. The more rapid is the shift in the distribution of power, the earlier the declining state stands firm and the greater the likelihood of war.

A more rapid shift in the distribution of power has two effects. The declining state becomes weaker faster, and the rising state becomes stronger faster. To formalize these effects in terms of the states' payoff functions, recall that D's payoff to fighting is $W_D(t) = 1/2 - dt$. As noted above, the parameter d has a natural interpretation. Formally, d is the rate at which the declining state's payoff to fighting is decreasing. More substantively, d is the rate at which the declining state's position or power is deteriorating and at which the rising state's power is growing. Thus, the larger is d, the faster D's power is declining. Conversely, the larger is d, the faster R's power and hence its payoff to fighting are rising. To formalize this effect, let $W_R(t, r) = r + dt$. An increase in d, therefore, increases W_R.

The effects of an increase in the rate at which power shifts from the declining to the rising state will be examined for the case of continual decline. (Parallel results can be obtained for the eventual-improvement case.) Although there is a multiplicity of equilibria in the continual-decline case, one of them seems especially focal. D's payoff in the equilibrium in which it initially stands firm at f_0 is the payoff to settling peacefully at f_0 weighted by the probability of doing so plus the payoff of fighting at f_0 multiplied by the probability that the rising state's minimal demands are not satisfied at f_0. In symbols, D's standing firm at f_0 yields $P_D(f_0)\Pr\{r : \bar{t}_r \le f_0\} + W_D(f_0)\Pr\{r : \bar{t}_r > f_0\}$. Let f_0^* be the stopping time that maximizes this payoff. Then, the equilibrium in which the declining state stands firm at f_0^* is focal in the sense that D stands firm at the optimal place, that is, the time that maximizes its equilibrium payoff.

A more rapid shift makes war more likely in this focal equilibrium if the probability of war goes up as d increases. An increase in d affects the probability of war through two channels. First, the distribution of R's breakeven times will shift out to the right. This means that the probability of war will rise even if the time at which D initially stands firm in the focal equilibrium is unaffected by the speed of the transition. That is, if the distribution of breakeven times shifts to the right and D stands firm at the same time, then the probability that R's minimal demands are satisfied when D stands firm decreases.

To see why faster transitions shift the distribution of breakeven times to the right, recall that r is just indifferent between fighting and settling peacefully at its breakeven time, which is now given by $\bar{t}_r(d) = r/(p - d)$. If, therefore, d increases to d', then R's payoff to fighting at $\bar{t}_r(d)$ will now be larger than its payoff to settling peacefully. Put another way, R's minimal demands no longer are satisfied at $\tilde{t}_r(d)$ if power is shifting at the rate d'. In order to satisfy these demands, D must concede more, because the rate at which D makes concessions, that is, the value of c, is fixed exogenously in the simple timing game, the fact that it takes more to satisfy r's minimal demands means that r's breakeven time is later, $\tilde{t}_r(d') > \tilde{t}_r(d)$. More directly, $\tilde{t}_r(d) = r/(p - d)$ is increasing in d.

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[22] To see that the sequence is decreasing, note that $\phi_0 - \phi_1 = [1 - 1/(2d)]\phi_0 - [\bar{t}_{\bar{r}} - 1/(2d)]$. Recalling that $f^* = (1 - 2d\bar{t}_{\bar{r}})/(1 - 2d)$, eliminating $\bar{t}_{\bar{r}}$ leaves $\phi_0 - \phi_1 = [1/(2d) - 1](f^* - \phi_0)$, which is positive because $d < 1/2$ and $f^* > \phi_0$. Similarly, $\phi_1 - \phi_2 = [1/(2d) - 1](f^* - \phi_1)$, and so on. Given that $\{\phi_k\}$ is strictly decreasing, it either converges to a ϕ^* or is unbounded. If the sequence were to converge, the ϕ^* would solve $\phi^* - \phi^* = [1/(2d) - 1](f^* - \phi^*)$ or $f^* = \phi^*$. But this is impossible, as all ϕ_k are less than f^*. Thus, $\{\phi_j\} \rightarrow -\infty$.

[23] See Kugler and Lemke (1996) for a recent review of power-transition theory.

Faster transitions, however, do affect the time at which the declining state stands firm. The faster the transition, the earlier D stands firm.[24] This second effect of a faster transition reinforces the first. Even if the distribution of R's breakeven times does not shift out, D's standing firm earlier means that fewer r are satisfied when D stands firm, and the probability of war is higher. Faster transitions are more dangerous in the model.[25]

CONCLUSION

A fundamentally important issue in international relations theory is understanding how states cope with shifts in the distribution of power in an environment in which they are uncertain about the ambitions of other states. The combination of a shifting distribution of power and uncertainty about the rising state's ambitions creates an inescapable trade-off. The earlier the declining state draws the line, the stronger it is and the higher its expected payoff in the event of war. But the earlier it draws the line, the higher the probability of an unnecessary war. That is, the earlier the declining state stands firm, the greater are the chances that it will fight a rising state whose demands are sufficiently limited that the declining state would have preferred satisfying them to fighting *had it known the extent of those demands.*

The present analysis has examined this trade-off in the simplest possible setting, which highlights the dilemma these two opposing factors create. In equilibrium, the declining state must balance these two opposing factors and generally does so by adopting a strategy or policy of appeasement. It makes a series of concessions in the hope of satisfying the other state but eventually draws a line and is unwilling to concede more. If these concessions satisfy the other state, the confrontation ends peacefully. If not, war results. In pursuing this policy, the declining state necessarily accepts some risk that there will eventually be a fight and that it will occur on less favorable terms. This is the unavoidable price that the declining state must pay for the chance of averting an unnecessary war.

Expectations play a crucial role in the states' interaction. Given that the declining state is always vulnerable to salami tactics, its credibility in standing firm depends on the expectation that if it does not, then the rising state will eventually make so many more demands that the declining state is better off taking a firm stand now rather than granting that long series of demands. In situations in which no environmental features serve to anchor these future expectations, almost anything can happen. This was the outcome of the continual decline case. When nothing pins down expectations, the declining state may stand firm at any time, depending on what it believes about the rising state's future demands. By contrast, in the case of eventual improvement, the fact that the declining state's military position bottoms out served as an expectational anchor. Reasoning backward from this endpoint leads to a unique equilibrium.

Finally, more rapid power transitions make war more likely in the model.

APPENDIX

This appendix defines the game discussed above, states the results formally, and outlines the proofs of those results. Because of limited space, only parts of the argument can be presented in full. The complete proofs are available from the author.

A strategy for the declining state is a function that specifies what D will do at any time $t \in [0, 1]$. That is, D's strategy is a function $\sigma_D: [0, 1] \rightarrow \{C, S\}$, where C stands for continuing and S for stopping.[26] Similarly, a strategy for the rising state in the incomplete-information game is a function $\sigma_R: [0, 1] \times [\underline{r}, \bar{r}] \rightarrow \{C, S\}$.

To formalize the declining state's beliefs, let $\theta(r)$ be any probability distribution over $[\underline{r}, \bar{r}]$, and let Θ be the set of all such distributions. Then, a belief system for D is a mapping $B_t: [0, 1] \rightarrow \Theta$, where B_t represents D's beliefs at time t about R's type, and B_0 is D's prior belief about R's resolve.

The general idea behind specifying the states' payoffs is that the situation ends in war if D stops first and ends in peace otherwise. More formally, let $q(t_0, r)$ be the time at which r first quits after t_0. In symbols, $q(t_0, r) = \inf\{t : t \geq t_0 \text{ and } \sigma_R(t, r) = S\}$.[27] Then, D's payoff to standing firm at a $t \geq t_0$ conditional on play having reached t_0 is

$$\begin{aligned} U_D(t, \sigma_R | t_0) = & \int_{\{r:q(t_0,r)<t\}} P_D(q(t_0, r))\, dB_{t_0} \\ & + P_D(t) \cdot \Pr\{r : q(t_0, r) = t \text{ and } \sigma_R(q(t_0, r), r) = S\} \\ & + W_D(t) \cdot \Pr\{r : q(t_0, r) = t \text{ and } \sigma_R(q(t_0, r), r) = C\} \\ & + W_D(t) \cdot \Pr\{r : q(t_0, r) > t\}. \end{aligned}$$

The first term is D's payoff due to the types of R that stop strictly before t. The second term is D's payoff due to the r's that actually stop at t. The third term is D's payoff due to those r that stop "at the next instant after t" but do not actually stop at t, that is, those r for whom $q(t_0, r) = t$ but $\sigma_R(t, r) = C$. Finally, the last term is D's payoff from those r that stop strictly later than t.[28]

If D stops at t conditional on play being at t_0, then r's payoff is $P_R(q(t_0, r))$ if $q(t_0, r) < t$. As long as r actually stops at t, r also receives $P_R(q(t_0, r))$ if $q(t_0, r) = t$. In all other circumstances r's payoff is $W_R(t, r)$.

The game will be analyzed in terms of pure-strategy PBE. A PBE for this game is an assessment (σ_D, σ_R, B) satisfying two conditions. First, the strategies $\sigma_R(t, r)$ and $\sigma_D(t)$ are best responses to each other at all times t, given D's beliefs, B_t. Second, D's beliefs at all $t \in (0, 1]$ are derived from D's prior beliefs, B_0; R's strategy, σ_R; and Bayes rule.[29]

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[24] In symbols, $\partial f_0^*/\partial d < 0$. Since $f_0^*(d)$ maximizes D's payoff, the Envelope Theorem can be used to show that $\partial f_0^*/\partial d < 0$.

[25] This conclusion should not be pushed too hard, as it may be an artifact of the assumption inherent in the simple timing-game formulation that the rate at which D makes concessions is exogenous.

[26] D will be assumed to stand firm there, that is, $\sigma_D(1) = S$, since standing firm then is weakly dominant.

[27] In order to ensure that the declining state's expected payoff is well defined, $q(t_0, r)$ is assumed to be a measurable function of r for all t_0.

[28] The second and third terms in the expression for D's payoff deal with the technicality that although the infimum of r's stopping times equals t, r may not actually stop at t. The payoff in this situation must be defined in order to specify the model. The equilibrium outcomes do not depend on how this technicality is handled, however. Similarly, strategies in which D stops at the "next instant after t," that is, $\sigma_D(t) = C$ but $t = \inf\{x : \sigma_D(x) = S \text{ and } x \geq t_0\}$, lead to equilibrium outcomes identical to those in which D stops at t. Therefore, these "next instant" strategies will be ignored.

Finally, it turns out to be more convenient in analyzing the game to work with the distribution of times, $\bar{t}_r$, at which the rising state's minimal demands have just been satisfied than with the distribution of the rising state's resolve. Indeed, this distribution of breakeven times is sufficient for the following analysis. To specify this distribution, let $Z(t)$ denote the cumulative distribution of $\bar{t}_r$. Then, $Z(t) = \Pr\{r : \bar{t}_r \leq t\} = \Pr\{r : W_R(t, r) \leq P_R(t)\}$.

The continuous-time specification raises a modeling issue. As will be seen, multiple equilibria exist in the continuous-time formulation. If, however, time is discrete and the players simultaneously decide whether to quit or continue, and if equilibria using weakly dominated strategies are disregarded, then this discrete-time analogue of the continuous-time game has a unique subgame-perfect equilibrium. Apparently, moving from discrete to continuous time "creates" the multiplicity of equilibria, and this may suggest that a better modeling approach is to use a discrete-time formulation. Yet, solving the problem of a multiplicity of equilibria by modeling the interaction in discrete time creates another, related problem. The equilibrium stopping time in a discrete-time game, although unique, is extremely sensitive to the time between decisions; a small change in that interval may lead to large changes in the equilibrium stopping time.

More formally, let Δ denote the time between decisions in a discrete-time game, and take $f_0(\Delta)$ to be the unique initial equilibrium stopping time in the game, with an amount of time Δ between decisions. Then, it would be natural to try to approximate a continuous-time game by letting Δ go to zero and taking $\lim_{\Delta \to 0} f_0(\Delta)$ to be the initial stopping time. But $\lim_{\Delta \to 0} f_0(\Delta)$ does not exist. Indeed, for any plausible stopping time f^* in the continuous-time game, there exists a sequence, $\Delta_1, \Delta_2, \Delta_3, \ldots$, such that $\Delta_k \to 0$, and $f_0(\Delta_k) \to f^*$.

PROPOSITION 1. *Assume: (i)* D*'s payoff functions,* $\mathrm{W_D(t)}$ *and* $\mathrm{P_D(t)}$*, are continuous over* [0, 1]*; (ii)* $\mathrm{W_D(t)}$ *is strictly decreasing over this interval; (iii)* $\mathrm{P_D(t) > W_D(t)}$ *for* $0 \leq \mathrm{t} < 1$*, with* $\mathrm{P_D(1) = W_D(1)}$*; and (iv)* $\mathrm{f_0}$ *is any plausible initial stopping time for the declining state. That is,* $\mathrm{f_0}$ *must be at least as large as the time at which the rising state's minimal demands are satisfied,* $\bar{\mathrm{t}}_\mathrm{r}$*, and* $\mathrm{P_D(f_0) \geq W_D(0)}$*. Then, the following strategies form a subgame perfect equilibrium:* D *stands firm and* R *stops making demands at each* $\mathrm{f_k}$ *and at* $\mathrm{t} = 1$*, where the sequence* $\{\mathrm{f_k}\}_{\mathrm{k}=0}^{\infty}$ *is generated by* $\mathrm{P_D(f_{k+1}) = W_D(f_k)}$*. At all other times* R *continues to make demands to which* D *continues to accede.*[30]

Proof. The proof follows immediately from the proof of Proposition 2 and is omitted.

PROPOSITION 2. *Assume: (i)* $\mathrm{W_D(t)}$ *and* $\mathrm{P_D(t)}$ *are continuously differentiable over* [0, 1]*, with* $\mathrm{W_D}$ *strictly decreasing; (ii)* $\mathrm{P_D(t) > W_D(t)}$ *over* [0, 1)*, with* $\mathrm{P_D(1) = W_D(t)}$*; (iii)* $\mathrm{P_D}$*,* $\mathrm{W_D}$*, and* Z *are real analytic functions at all* $\mathrm{t} \in (\max\{0, \bar{\mathrm{t}}_\mathrm{r}\}, 1]$[31]*; and (iv)* $\mathrm{f_0} \geq 0$ *is any plausible initial stopping time, that is,* $\mathrm{P_D(f_0)Z(f_0) + W_D(f_0)[1 - Z(f_0)] \geq P_D(0)Z(0) + W_D(0)(1 - Z(0))}$*. Then, generically there exists a PBE in which the declining state initially stands firm at* $\mathrm{f_0}$*. Types of the rising state whose minimal demands are satisfied by* $\mathrm{f_0}$*, that is, those* r *for which* $\bar{\mathrm{t}}_\mathrm{r} \leq \mathrm{f_0}$*, continue until* $\mathrm{f_0}$*, where they stop making further demands. Other types do not stop and fight the declining state at* $\mathrm{f_0}$*.*

Proof. The proof of the proposition is in two parts. Recall that in the case with linear payoffs and a uniform distribution over r, the declining state stops at $f_0, f_1, \ldots$ and at all points in an interval $[f^*, 1]$. The first part of the proof is to generalize the procedure used to construct this set in the linear case and then use this generalized procedure to derive a set of stopping times in the more general case. This set of stopping times will turn out to be composed of an infinite sequence of points and a finite number of closed intervals. The second step in the argument is to show that a PBE is formed by strategies of D's stopping at each point in this set and of r's stopping at each point in this set as soon as its minimal demands are satisfied.

In order to generalize the procedure for constructing the set of stopping times, it is useful to introduce some notation and establish four lemmas. Let $G(t|\tau)$ be D's payoff to standing firm at t conditional on play having reached τ, and given that the types of r D is facing are those with breakeven times of τ or later, that is, $G(t|\tau) = P_D(t)[(Z(t) - Z(\tau))/(1 - Z(\tau)] + W_D(t)[(1 - Z(t))/(1 - Z(\tau)]$, where, recall, $Z(t)$ is the distribution of breakeven times $\bar{t}_r$.

The function G provides a compact and convenient way of writing equation 1. As in the linear case, D's decision to stand firm at a time f_k will be rationalized by the expectation that if it does not, then the rising state will continue making demands until f_{k+1}, where, according to equation 1, D's payoff to fighting at f_k is just equal to its payoff to waiting until f_{k+1} to stand firm, conditional on facing only those types of r with breakeven times of at least f_k. Writing this condition in terms of G means that f_k and f_{k+1} satisfy $W_D(f_k) = G(f_{k+1}|f_k)$.

It will also be convenient to have an expression for the difference between $G(t|\tau)$ and D's payoff to fighting at τ. Let $\Delta(t|\tau) = G(t|\tau) - W_D(\tau)$. If, therefore, f_k and f_{k+1} are two adjacent stopping times in the linear case, then $\Delta(f_{k+1}|f_k) = 0$. The first lemma shows that this relation holds in general.

LEMMA 1A. *Let* $\tau_0 < \mathrm{t_0}$ *be adjacent equilibrium stopping times, that is,* D *stops at* τ_0 *and* $\mathrm{t_0}$ *and continues at all intermediate times. Then,* $\Delta(\mathrm{t_0}|\tau_0) = 0$.

Proof. Consider D's decision at τ_0, and let t be the time at which D last stood firm: $t = \sup\{t : t < \tau_0 \text{ and } \sigma_D(t) = S\}$. D's payoff to standing firm at τ_0 is $P_D(\tau_0)[(Z(\tau_0) - Z(t))/(1 - Z(t)] + W_D(\tau_0)[(1 - Z(\tau_0))/(1 - Z(t)]$. If D's next equilibrium stopping time is t_0, then r will also stop at t_0 if $\tau_0 < \bar{t}_r \leq t_0$. Accordingly, D's payoff to waiting until t_0 to stand firm is $P_D(\tau_0)[(Z(\tau_0) - Z(t))/(1 - Z(t)] + P_D(t_0)[(Z(t_0) - Z(\tau_0))/(1 - Z(t)] + W_D(t_0)[(1 - Z(t_0))/(1 - Z(t)]$. The first term in this expression is D's payoff due to those r who stop at τ_0, the second is due to those who stop at t_0, and the third is due to those who continue beyond t_0. The difference between D's payoff to stopping at τ_0 and t_0 is $\Delta(t_0|\tau_0)[(1 - Z(t_0))/(1 - Z(t)]$. D strictly prefers to continue at τ_0 if $\Delta(t_0|\tau_0) > 0$, and consequently τ_0 is not an equilibrium stopping time. So, $\Delta(t_0|\tau_0) \leq 0$. Indeed, $\Delta(t_0|\tau_0)$ cannot be strictly negative. If it were, then continuity would ensure that $\Delta(t_0|\tau_0') < 0$ for a τ_0' slightly larger than τ_0. But $\Delta(t_0|\tau_0') < 0$ implies that, conditional on being at τ_0', D strictly prefers to stand firm there rather than continue to t_0, which contradicts the assumption that τ_0 and t_0 are adjacent stopping times. Hence, $\Delta(t_0|\tau_0) = 0$ for adjacent stopping times. Q.E.D.

Although Δ constrains the set of possible stopping times, these times will actually be generated by two other functions. Roughly, let $g(\tau)$ be the latest time that may be an equilibrium stopping time which is adjacent to τ. That is, $g(\tau) = \max\{t : \Delta(t|\tau) = 0\}$ for $\tau \in [0, 1)$, and take $g(1) = 1$. This function is well defined, as the set $\{t : \Delta(t|\tau) = 0\}$ is closed, bounded, and nonempty by virtue of containing τ. Note further that $g(\tau)$ is bounded above by one for any $\tau \leq 1$, since $\Delta(t|\tau) \leq 0$ for all $t \geq 1$.

Intuitively, g maps τ into the potentially adjacent stopping time that is as late as possible. The second function, h, is a kind of inverse of g, mapping a time t into an earlier adjacent stopping time, τ. The function h is also defined so that $\Delta h(t/h(t))$ is strictly positive between $h(t)$ and t, a property which will be important in what follows. Formally, $h(t) = \inf\{\tau : \tau \geq 0 \text{ and } \Delta(t|\tau') > 0 \text{ for } \tau' \in (\tau, t)\}$. Assuming, vacuously, that t is in the set $\{\tau : \tau \geq 0 \text{ and } \Delta(t|\tau') > 0 \text{ for } \tau' \in (\tau, t)\}$, then this set is nonempty, the infimum exists, and, therefore, h is well defined. It follows immediately from the definition of h that $\tau = h(t)$ is a root of $\Delta(t|\tau)$, that is, $\Delta(t|h(t)) = 0$ if $h(t) > 0$.

The set of stopping times is composed of an infinite series (actually several infinite series) of isolated stopping times as well as a finite number of intervals. The functions g and h will be used to construct the isolated stopping times. To define the intervals, let V be the set of times t, such that D's payoff to standing firm at t is at least as large as its payoff to continuing and standing firm an "instant later." As will be seen, D can and will stop in equilibrium at all the points in V because it never pays to continue.

To characterize V more formally, note that D's payoff to standing firm at t is at least as large as its payoff to standing firm at $t + \delta$ if $\Delta(t + \delta|t) \leq 0$ or, equivalently, if $W_D(t) \geq W_D(t + \delta) + (P_D(t +$

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[29] Those types r whose minimal demands are not satisfied by $t = 1$, that is, for whom $\bar{t}_r > 1$, will never quit. Accordingly, Bayes's rule can be applied at all times t and used to specify D's beliefs.

[30] There are also other equilibria. If, for example, $\bar{t}_r = 0$, then D and R may stop at all times.

[31] A function $y(t)$ is a real analytic function at t' if there exists an open interval containing t', such that y can be expressed as a power series in this interval. See Rudin (1964, 158–61) for a discussion of some of the properties of these functions.

$\delta) - W_D(t + \delta))(Z(t + \delta) - Z(t))/(1 - Z(t))$. Dividing through by δ and taking the limit as δ goes to zero leave $0 \geq W'_D(t) + (P_D(t) - W_D(t))z(t)/(1 - Z(t))$. Accordingly, V is the set of times in $[\tilde{t}_r, 1]$ for which $0 \geq W'_D(t) + (P_D(t) - W_D(t))z(t)/(1 - Z(t))$.[32]

Finally, it will be useful to let $\nu(\tau)$ be the slope of $\Delta(t|\tau)$ at τ for $\tau \in [\tilde{t}_r, 1]$. That is,

$$\nu(\tau) = \frac{\partial \Delta(t|\tau)}{\partial t}\bigg|_{t=\tau} = W'_D(\tau) + (P_D(\tau) - W_D(\tau))\,\frac{z(\tau)}{1 - Z(\tau)}.$$

Clearly, ν can be used to indicate when a time τ is in V. To wit, $\tau \in V$ if and only if $\nu(\tau) \leq 0$.

The following three lemmas establish useful properties about g, h, and V.

LEMMA 2A. *If* $\tau \notin \mathrm{V}$, *then* $\mathrm{g}(\tau) > \tau$.

Proof. Since $\tau \notin V$, $\nu(\tau) > 0$. But $0 < \nu(\tau) = \partial\Delta/\partial t|_{\tau=t}$ implies $\Delta(t'|\tau) > 0$ for a t' slightly larger than τ. And $\Delta(1|\tau) = W_D(1) - W_D(\tau) < 0$. The Intermediate Value Theorem then ensures that there is a $t^* > t' > \tau$, such that $\Delta(t^*|\tau) = 0$, which leaves $g(\tau) \geq t^* > \tau$. *Q.E.D.*

LEMMA 3A. *If* $\mathrm{h(t)} > 0$, *then* $\Delta(\mathrm{t}|\mathrm{h(t)}) = 0$. *Furthermore, if* $\mathrm{t} \in [\tilde{\mathrm{t}}_\mathrm{r}, 1]$ *but* $\mathrm{t} \notin \mathrm{V}$, *then* $\mathrm{h(t)} < \mathrm{t}$.

Proof. Suppose $\Delta(t|h(t)) > 0$ and $h(t) > 0$. Then, there would exist at $t' \in (0, h(t))$ such that $\Delta(t|\tau) > 0$ for $\tau \in (t', t)$, which contradicts the definition of $h(t)$. If $\Delta(t|h(t)) < 0$, then there exists a t' such that $t < t' < h(t)$ and $\Delta(t|t') < 0$, which again contradicts the definition of h. Thus, $\Delta(t|h(t)) = 0$ if $h(t) > 0$.

If $t \in [\tilde{t}_r, 1]$ and $t \notin V$, then $\nu(t) > 0$. But $-\nu(t) = \partial\Delta(t|\tau)/\partial\tau|_{\tau=t}$, so $\partial\Delta(t|\tau)/\partial\tau|_{\tau=t} < 0$. Continuity and the fact that $\Delta(t|t) = 0$ then ensure that there exists an $\varepsilon > 0$, such that $\Delta(t|\tau) > 0$ for $\tau \in (t - \varepsilon, t)$, which leaves $h(t) \leq t - \varepsilon < t$. *Q.E.D.*

LEMMA 4A. *If* $\mathrm{x} > \max\{0, \tilde{\mathrm{t}}_\mathrm{r}\}$, *then* $\mathrm{V} \cap [\mathrm{x}, 1]$ *can be written as the union of finitely many disjoint closed intervals, that is, there exists a finite set* $\mathrm{t}_0 \leq \mathrm{t}_1 \leq \cdots \leq \mathrm{t}_\mathrm{K}$, *such that* $\mathrm{V} \cap [\mathrm{x}, 1] = [\mathrm{t}_0, \mathrm{t}_1] \cup [\mathrm{t}_2, \mathrm{t}_3] \cup \cdots \cup [\mathrm{t}_{\mathrm{K}-1}, \mathrm{t}_\mathrm{K}]$ *and* $[\mathrm{t}_\mathrm{j}, \mathrm{t}_{\mathrm{j}+1}] \cap [\mathrm{t}_\mathrm{k}, \mathrm{t}_{\mathrm{k}+1}] = \emptyset$ *for* $\mathrm{j} \neq \mathrm{k}$.

Proof. This follows directly from the properties of real analytic functions. See, for example, Rudin (1964, 162–3). *Q.E.D.*

These preliminary definitions and lemmas make it possible to start with any positive plausible stopping time f_0 and then construct a set of stopping times that will rationalize this initial stopping time in a PBE. (The case in which $f_0 = 0$ is a straightforward extension of the argument when $f_0 > 0$.) The first step is to use f_0 as a seed for the sequence $\{f_k\}_{k=0}^{\infty}$, where $f_{k+1} = g(f_k)$. This sequence is nondecreasing, since $g(t) \geq t$ and, as noted above, is bounded by one. Thus, $\{f_k\}$ converges to an $f_0^* \leq 1$. The sequence $\{f_k\}_{k=0}^{\infty}$ defines D's stopping times in the interval $[0, f_0^*)$.

To describe D's actions in $[f_0^*, 1)$ (recall that D stops at $t = 1$), note first that $f_0^* > \tilde{t}_r$. To see this, observe that the facts that f_0 is plausible and positive implies that $f_0 > \tilde{t}_r$. But $f_0^* \geq f_0$, which leaves $f_0^* > \tilde{t}_r$.

Moreover, $f_0^* \in V$. To verify this, suppose that g is not strictly increasing. Then, there exists a K such that $g(f_k) = f_k$ for all $k \geq K$. Hence, $f_0^* = f_K$, and lemma 2A implies $f_0^* \in V$, since $g(f_0^*) = f_0^*$. Now suppose that g is strictly increasing. Since g is strictly increasing, $f_k < f_{k+1}$. But $\Delta(f_k|f_k) = \Delta(f_{k+1}|f_k) = 0$ by construction. Thus, there must exist an $m_k \in [f_k, f_{k+1}]$ such that $\partial\Delta(t|f_k)/\partial t|_{t=m_k} = 0$. Taking the limit as $k \to \infty$ gives $\partial\Delta(t|f_0^*)/\partial t|_{t=f_0^*} = 0$. But $\nu(f_0^*) = -\partial\Delta(t|f_0^*)/\partial t|_{t=f_0^*} = 0$, which implies $f_0^* \in V$.

Since $f_0^* \in V$, there exists an upper bound of the component of V containing f_0^*. Let s_0^* be this upper bound. Then, $[f_0^*, s_0^*] \subset V$, and $[f_0^*, s_0^* + \varepsilon] \not\subset V$ for any $\varepsilon > 0$. D will be assumed to stop at all $t \in [f_0^*, s_0^*]$.

The specification of D's actions in $[0, 1]$ is complete if $s_0^* = 1$. (This was the case examined when the functions P_D and W_D were assumed to be linear and Z was uniform.) If $s_0^* < 1$, then D's stopping times in $(s_0^*, 1)$ remain to be specified.

Suppose a procedure can be found for specifying D's stopping times in an interval $(s_0^*, f_0^1]$, where $f_0^1 > s_0^*$. Then f_0^1 can be used as a new seed for a sequence $\{f_k^1\}$, such that $f_{k+1}^1 = g(f_k^1)$. Arguing as above, this sequence converges to an $f_1^* \in V$. As before, D will stop at each f_{k+1} and at all $t \in [f_1^*, s_1^*]$ and continue at all other times in $(f_0^1, s_1^*]$. If $s_1^* = 1$, then D's stopping times in $[0, 1]$ will have been specified. If $s_1^* < 1$, then the hypothesized procedure can be used again to specify D's stopping times in $[s_1^*, f_0^2]$, where $f_0^2 > s_1^*$. Now f_0^2 can be used as a new seed for a sequence $\{f_k^2\}$, such that $f_{k+1}^2 = g(f_k^2)$, and so on.

Lemma 4A shows that $V \cap [f_0, 1]$ only has finitely many components. Moreover, each time the procedure hypothesized in the previous paragraph is repeated, it "jumps over" an upper bound of a component of V. That is, $f_k^* \leq s_k^* < f_0^{k+1}$. Thus, repeating the procedure a finite number of times eventually yields an $s_n^* = 1$, in which case D's standing firm at all $t \in [f_n^*, s_n^*]$ completes the specification of D's stopping times at all $t \in [0, 1]$.

Turning to the procedure for determining D's stopping times in an interval $(s_0^*, f_0^1]$ where $f_0^1 > s_0^*$, suppose $g(s_0^*) > s_0^*$. Then, intuitively D can credibly commit itself to standing firm at s_0^* if $g(s_0^*)$ is also a stopping time, since $\Delta(g(s_0^*)|s_0^*) = 0$. Accordingly, take $f_0^1 = g(s_0^*)$ if $g(s_0^*) > s_0^*$, and assume D stops at f_0^1 and continues at all $t \in (s_0^*, f_0^1)$.

If $g(s_0^*) = s_0^*$, then it is easy to rationalize D's standing firm at s_0^* since the definition of g and the assumption that $g(s_0^*) = s_0^*$ imply $\Delta(t|s_0^*) < 0$ for all $t > s_0^*$. It is more difficult to specify D's actions at the "next instant" after s_0^*. The idea is to show that there exists an $s_0 > s_0^*$ and a sequence $\{s_k\}$ generated by $s_{k+1} = h(s_k)$ which is strictly decreasing and converges to s_0^*. D, then, will be assumed to stop at each s_k and continue at all other times in (s_0^*, s_0). Furthermore, s_0 will be used as a seed for $\{f_k^1\}$; that is, $f_0^1 = s_0$.

To see that such a sequence $\{s_k\}$ exists, take ε small enough that $(s_0^*, s_0^* + \varepsilon) \cap V = \emptyset$. Lemma 4A ensures that such an ε exists. As long as $s_k \in (s_0^*, s_0^* + \varepsilon)$, $s_{k+1} < s_k$. This follows from lemma 3A and the facts that $\tilde{t}_r < f_0 \leq s_0^* < s_k$ and $s_k \notin V$. If $s_k = s_0^*$ for some k, then the construction is complete. Assume, therefore, that $s_k \neq s_0^*$ for all k. Then, $\{s_k\}$ is bounded below by s_0^*, that is, $h(s_k) \geq s_0^*$. Suppose not; there then exists an n such that $h(s_n) < s_0^* < s_n$. But $h(s_n) < s_0^*$ implies $\Delta(s_0^*|s_n) > 0$, since, by construction, $\Delta(s_n|\tau) > 0$ for all $\tau \in (h(s_n), s_n)$. But $\Delta(s_n|s_0^*) > 0$ means that there exists a $t > s_n$ such that $\Delta(t|s_0^*) = 0$ and, therefore, that $g(s_0^*) \geq t > s_n \geq s_0^*$, which contradicts the fact that $g(s_0^*) = s_0^*$.

Since $\{s_k\}$ is strictly decreasing and bounded below by s_0^*, s_k must converge to a $t^* \geq s_0^*$. Suppose $t^* > s_0^*$. Since $s_0^* < t^* < s_k < s_0^* + \varepsilon$, it follows that $t^* \notin V$ and $s_k \notin V$. Consequently, $\nu(s_k) > 0$. But as noted in the proof of lemma 3A, $-\nu(t) = \partial\Delta(t|\tau)/\partial\tau|_{\tau=t}$, which leaves $\partial\Delta(t|\tau)/\partial\tau|_{\tau=t} < 0$ at $\tau = s_k$. Thus, there must exist an $m_k \in [h(s_k), s_k]$ such that $\partial\Delta(s_k|\tau)/\partial\tau = 0$ at $\tau = m_k$. Otherwise, $\partial\Delta(s_k|\tau)/\partial\tau$ would be negative over $[h(s_k), s_k]$ and, therefore, $\Delta(s_k|h(s_k)) > \Delta(s_k|s_k) = 0$. But $\Delta(s_k|h(s_k)) > 0$ contradicts lemma 3A, since $0 < s_0^* \leq h(s_k)$ and $\Delta(s_k|h(s_k)) = 0$. Accordingly, $\partial\Delta(s_k|\tau)/\partial\tau = 0$ at $\tau = m_k$ for an $m_k \in [h(s_k), s_k]$. Taking the limit leaves $0 = \partial\Delta(t^*|\tau)/\partial\tau|_{\tau=t^*} = \nu(t^*)$ and the contradiction that $t^* \in V$. Hence, $s_k \to s_0^*$.

Given the preceding method for characterizing D's stopping times in the interval (s_k^*, f_0^{k+1}), the entire procedure described above can be used to specify D's stopping times in the entire interval $[0, 1]$. Summarizing, D continues at all $t \in [0, f_0)$. D then stands firms at each f_k in $\{f_k\}$; at all times in the interval $[f_0^*, s_0^*]$; and subsequently at each s_k in $\{s_k\}$ if $g(s_0^*) = s_0^*$, or at $g(s_0^*)$ if $g(s_0^*) > s_0^*$. Repeating the cycle, D stands firm at each f_k^1 in $\{f_k^1\}$; at all t in $[f_1^*, s_1^*]$; and then at each s_k^1 in $\{s_k^1\}$ if $g(s_1^*) = s_1^*$, or at $g(s_1^*)$ if $g(s_1^*) > s_1^*$. Repeating the cycle again, D stops at each f_k^2 in $\{f_k^2\}$; at all t in $[f_2^*, s_2^*]$; and then at each s_k^2 in $\{s_k^2\}$ if $g(s_2^*) = s_2^*$, or at $g(s_2^*)$ if $g(s_2^*) > s_2^*$. Repeating this process a finite number of times eventually yields an N such that $s_N^* = 1$, which completes the specification of the stopping times in $[0, 1]$.

Now consider the strategies in which D stops at each of these times and continues at all other times and in which r continues at all $t < \tilde{t}_r$ and stops at all $t \geq \tilde{t}_r$, if and only if t is also a stopping time for D. The second part of the proof of proposition 2 is to show that these strategies are best responses to each other at all times $t \in [0, 1]$. Limited space precludes anything more than a sketch of the requisite steps.

Step 1. It is straightforward to show that $[f_N^*, 1] \subset V$. D's standing firm at $t = 1$ and the definition of V then imply that D also stands firm at all $t \in [f_N^*, 1)$.

Step 2. D has no incentive to deviate in the interval $[f_0^N, f_N^*)$. To establish this, suppose first that D is at a stopping time in this interval, that is, D is at a t such that $t = f_k^N$ for some k. By construction, D is indifferent between stopping at f_k^N and any later

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[32] If Z is not differentiable at the lower bound of its support, then $z(\tilde{t}_r)$ is not well defined. In this case, take $z(\tilde{t}_r)$ to be $\lim_{t \to \tilde{t}_r^+} z(t)$.

f^N_{k+j} and strictly prefers stopping at a stopping time to standing firm at any intermediate time. Now suppose that D is not at a stopping time in $[f^N_0, f^*_N)$. Then, it strictly prefers continuing to the next stopping time. Thus, standing firm at all $f^N_k \in [f^N_0, f^*_N)$ and continuing at all other times is a best response for D.

Step 3. Arguing as in step 2 shows that D's best response in $[s^*_{N-1}, f^N_0)$ is to stop at all s^{N-1}_k and continue at all other times.

Step 4. D stands firm at all $t \in [f^*_{N-1}, s^*_{N-1})$ because $[f^*_{N-1}, s^*_{N-1}) \subset V$ and D is standing firm at s^*_{N-1}.

Step 5. Repeating steps 1–4 shows that the states' strategies are best responses to each other over $[f^*_{N-2}, f^*_{N-1})$ and so on. *Q.E.D.*

PROPOSITION 3. *Assume (i)* ϕ_0 *is the time at which the declining state is indifferent as between its payoff to fighting at* ϕ_0 *and at* $t = 1$, *that is,* ϕ_0 *solves* $W_D(\phi_0) = W_D(1)$; *(ii)* $W_D(t) < W_D(1)$ *for* $\phi_0 < t < 1$; *(iii)* W_D *is strictly decreasing and upwardly concave over* $[0, \phi_0]$, *that is,* $W'_D < 0$ *and* $W''_D \geq 0$; *(iv)* Z*'s density is differentiable, and its hazard rate is nondecreasing over* $(\bar{t}_r, 1)$[33]; *and (v)* $\partial(G(t|\tau) - W_D(\tau))/\partial t|_{t=\tau} > 0$ *for all* $\bar{t}_r \leq t \leq \phi_0$.[34] *Then, (i) there exists a strictly increasing sequence,* $\phi_{N+1}, \phi_N, \ldots, \phi_0$, *such that* $\Delta(\phi_k|\phi_{k+1}) = 0$ *and* $N + 1$ *is the smallest integer* n, *such that* $\phi_n \leq \max\{0, t_r\}$; *(ii) there exists a unique PBE if* D*'s payoff to standing firm initially at* ϕ_N *is strictly greater than its payoff to standing firm at* $t = 0$. *In this equilibrium,* D *stands firm at* $\phi_N, \phi_{N-1}, \ldots, \phi_0$ *and at* $t = 1$ *and continues at all other times; (iii)* D*'s stopping times are dense in* $[0, \phi_{N+1}]$ *if* D*'s payoff to initially standing firm at* ϕ_N *equals its payoff to standing firm at* $t = 0$ *and if* $\phi_{N+1} \neq 0$. *After* ϕ_{N+1}, D *stands firm at* $\phi_N, \phi_{N-1}, \ldots, \phi_0$ *and at* $t = 1$ *and continues at all other times; and (iv) in both (ii) and (iii),* r*'s strategy is to continue at all* $t < \bar{t}_r$. *If* $t \geq \bar{t}_r$, *then* r *stops whenever* D *stands firm and continues whenever* D *continues.*

Proof (sketch): There are four major steps.

Step 1: D stops at $t = 1$ and continues at all $t \in (\phi_0, 1)$.

Step 2. D must stop at ϕ_0 in equilibrium. This follows from (i) the latest stop time in the closure of the set of stopping times less than or equal to ϕ_0 equals ϕ_0, that is, $\sup\{t : t \leq \phi_0 \text{ and } \sigma_D(t) = S\} = \phi_0$; and (ii) D's stop times are not dense in $[\phi_0 - \varepsilon, \phi_0]$ for any $\varepsilon > 0$.

Step 3. Assumptions (i)–(v) ensure $[\max\{0, \bar{t}_r\}, 1] \cap V = \emptyset$. Accordingly, the sequence $\{\phi_k\}$ generated by $\Delta(\phi_k|\phi_{k+1}) = 0$ and ϕ_0 exists and is strictly decreasing over $[\max\{0, \bar{t}_r\}, 1]$.

Step 4. The previous two steps then imply that the other equilibrium stopping times must satisfy $\Delta(\phi_k|\phi_{k+1}) = 0$. *Q.E.D.*

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[33] Bagnoli and Bergstrom (1989) show that the following distributions have log-concave densities and hence nondecreasing hazard rates: uniform, normal, logistic, extreme value, chi-squared, chi, exponential, and Laplace, and, for some parameter values, Wiebull, power function, gamma, and beta. See also Bergstrom and Bagnoli (1993, 195) and Fudenberg and Tirole (1991, 267) for further discussion.

[34] This assumption is a generalization of the requirement that $\phi_0 < f^*$, which was needed in the linear case described above. If $Z(t)$ is not differentiable at $\bar{t}_r$, then the partial derivative is evaluated at $\bar{t}_r$ by letting t approach $\bar{t}_r$ from the right.

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Do Bills of Rights Matter? The Canadian Charter of Rights and Freedoms

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Although constitutional protection for rights is increasingly popular, there is little systematic research on the extent to which bills of rights affect the process of government. This article examines the effects a bill of rights may be expected to produce, and then uses a quasi-experimental design to analyze the effects of the Canadian Charter of Rights and Freedoms on the Canadian Supreme Court's agenda. The data suggest that the Charter indeed has influenced the Court's agenda, although the effects are more limited than generally recognized. More important, the data suggest that a number of the influences often attributed to the Charter likely resulted instead from the growth of what I call the support structure for legal mobilization, consisting of various resources that enable litigants to pursue rights-claims in court. The political significance of a bill of rights, then, depends on factors in civil society that are independent of constitutional structure.

Constitutionalism, the attempt to protect liberty through the design of political institutions, is currently enjoying a revival of interest. Numerous countries, ranging from former members of the Eastern Bloc to Canada, have engaged in fundamental constitutional reform, and social scientists, returning to the study of institutions, have renewed the old wisdom that constitutions matter and matter greatly (see, e.g., Brennan and Hamlin 1994, Greenberg et al. 1993, Holmes 1995, Ordeshook 1992, Sartori 1994, Weingast 1993).

Nearly every new constitution or constitutional revision adopted since 1945 (almost 60 by rough count) contains a bill of rights. Some are shams, yet even those which are not are quite flexible in practice. The U.S. Bill of Rights, for instance, after being nearly ignored by courts for a century and a half, became a foundation for much judicial policymaking. The Indian Constitution's due process clause, which was drafted narrowly so as to preclude the development of American-style judicial activism, nonetheless is now the basis for much judicial creativity (Barnum 1988). Bills of rights, in short, are unpredictable components of constitutionalism.

To what extent, and under what conditions, will a bill of rights fulfill its promise to protect liberty? Answering that question is a difficult task, in part because what we may expect from constitutional reform remains all too often a matter of political rhetoric, not theoretical analysis, and in part because constitutional change is so often intimately bound up with regime change, and thus the effects of the former typically are not easily unraveled from their complex and often turbulent contexts. I present results of a preliminary analysis consisting of a theoretical discussion of the expected effects of a bill of rights and empirical research designed to isolate the actual effects from other influences.

My empirical analysis consists of a quasi-experiment of the effects of the Canadian Charter of Rights and Freedoms. The Charter, a constitutional bill of rights adopted in 1982, is a leading example of the recent revival of constitutionalism, and it offers a nearly ideal opportunity to test for the effects of constitutional change. When the Charter was adopted, Canada's political regime continued much as before; nevertheless, observers of Canadian politics have attributed to the Charter remarkably profound effects, particularly a growing emphasis on individual rights; and those claimed effects are relatively easily isolated and tested because they are expected to appear in a single institution, the Canadian Supreme Court. In short, Canada should be an easy case for the hypothesis that a bill of rights independently affects politics.

I shall argue, however, based on data from the agenda of the Canadian Supreme Court, that the effects of a bill of rights are not as direct as constitutional engineers and scholars commonly assume, because those effects depend on structural conditions, in particular the presence of what I shall call a support structure for legal mobilization, which varies greatly among countries, across time, and among issue areas. The support structure consists of resources—sympathetic and competent lawyers, finances, and organizations—that make possible sustained, strategic appellate litigation. The contours of such resources condition access to the higher courts and, therefore, condition the nature of the issues decided by those courts. Bills of rights matter but only to the extent that individuals can mobilize the resources necessary to invoke them through strategic litigation.

To demonstrate the importance of the support structure, I consider and test several alternative explanations of variations in judicial attention to civil liberties and civil rights. They are (1) the presence or absence of a bill of rights and (2) the justices' policy preferences, conditioned by (3) the extent of judicial discretion over the docket. Recent developments in Canadian constitutional and judicial politics, as I shall show, make possible a nearly ideal test of the alternative hypotheses. Although time series data could be used to test these hypotheses, due to limitations in their availability I have relied on triangulation, a strategy commonly used in qualitative research whereby multiple sources of data and multiple indicators are used to verify causal inferences (Jick 1979; King, Keohane, and Verba 1994, 217–8; Webb et al. 1981, 35, 315).

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CONSTITUTIONALISM AND THE CONDITIONS FOR APPLICATION OF A BILL OF RIGHTS

Constitutions, the most basic of political institutions, affect ordinary politics by prescribing the rules of the political game (March and Olsen 1989). Liberal constitutions typically employ two very different kinds of rules to constrain arbitrary power. Some rules structure governing institutions in such a way that decision making is channeled and thereby limited (this is the purpose of the separation of powers and federalism). Other rules define particular issues as outside the authority of the ordinary democratic process (this is the purpose of bills of rights) (Elster 1988, 3–4). Both types of rules, it may appear, produce similarly profound and direct effects on politics. But not all rules are created equal: Federalism and the separation of powers are self-activating, bills of rights are not. The differences between the two types of rules help to illuminate the conditions that influence application of a bill of rights.

Structural mechanisms, as James Madison explained in his classic argument in *The Federalist*, are self-activating because they tie individual interests to institutional resources. Thus, federalism divides factional conflict so that no single faction can succeed in gaining control of the national government, and the separation of powers, by tying the personal ambitions of officeholders to departmental interest, sets the competing governmental institutions against one another, each checking the power of the others and thereby contributing to deliberation (*Federalist*, Nos. 10, 45, 46, 51). These structural components of constitutionalism have appeared to some observers to work mechanically, as "a machine that would go of itself" (Lowell 1888, 312, quoted in Kammen 1986, 18;[1] for recent analyses, see Brennan and Hamlin 1994, Ordeshook 1992, Sartori 1994, Weingast 1993).

Bills of rights, by contrast, are not self-activating because, unlike the separation of powers, they provide individuals with no direct control over institutional resources. In other words, although bills of rights create legal interests (rights), they create no corresponding institutional resources to actualize those interests. Thus, in Madison's evocative metaphor, constitutional rights guarantees are mere "parchment barriers" (Madison 1977 [hereafter *PJM*] 10:211–2). Nonetheless, remarkably profound effects are often attributed to bills of rights. I survey those hypothesized effects and then turn to the conditions—judicial attitudes, discretionary docket control, and external support for legal mobilization—that are likely to influence the application of bills of rights in practice.

Bills of Rights

Bills of rights are often thought to affect politics in several ways. First, it is commonly suggested that they promote an emphasis on rights in political culture (Hart 1994, 174; *PJM* 11:298–9, 12:204–5). Similarly, some scholars argue that rights guarantees shape the development of political and social movements (Hartog 1987; see also McCann 1994).

Second, observers commonly suggest that bills of rights increase the level of intervention by courts in the policymaking processes of other governmental institutions. Thus, Madison speculated that a bill of rights would "naturally" lead courts to "resist every encroachment upon rights" by legislatures and executives (*PJM* 12:207). Similarly, contemporary scholars often trace the extraordinary vibrancy of judicial review, judicial attention to rights, and judicial policymaking on rights in the United States to the presence of a bill of rights in the U.S. Constitution (Atiyah and Summers 1987, 238–9; Holland 1991).

Third, contemporary writers have suggested that the presence of a bill of rights, by encouraging a broader policymaking role for the judiciary, opens the political opportunity structure and thereby encourages a fragmentation of broad coalitions and parties into numerous competing interest groups, which abandon collective solutions in the legislature for more individualized solutions in the courts (Knopff and Morton 1992, Morton 1987).

There are reasons to be skeptical, however, that the mere presence of a bill of rights will have much effect. Madison, for instance, wrote to Jefferson that "experience proves the inefficacy of a bill of rights on those occasions when its controul is most needed.... I have seen the bill of rights violated in every instance where it has been opposed to a popular current" (*PJM* 10:211–2; Rakove 1991, 136).

The Support Structure for Legal Mobilization

The most important weakness of a bill of rights, as I previewed above, is that, unlike the self-enforcing structure embodied in federalism and the separation of powers, a bill of rights creates no automatic institutional resources for its own enforcement. Although bills of rights commonly empower at least some courts to redress violations of rights, there remain several weaknesses to the machinery of enforcement. Some scholars emphasize what they believe to be courts' relatively weak enforcement powers (Rosenberg 1991). Perhaps as important, however, is that rights guarantees rarely provide potential litigants with the resources necessary to mobilize the law. As Madison speculated, judicial enforcement of a bill of rights will be ineffective "*particularly . . . where the law aggrieves individuals, who may be unable to support an appeal agst. [against] a State to the supreme Judiciary*" (*PJM* 10:211–2, emphasis added; Rakove 1991, 136). Contemporary scholars reach a similar conclusion. The legal system functions as an entrepreneurial market in which development of law is affected by individuals' decisions to mobilize the law (Zemans 1983); and decisions to mobilize the law depend on individuals' capacity to do so, which depends partly on their access to resources (Galanter 1974, Lawrence 1990, Olson 1990).

At the level of national supreme courts, in particular, cost is an important barrier to access (Lawrence 1990, 3–8). The cost of taking any particular case to a supreme court is substantial. The total cost of getting an issue onto such a court's agenda includes the cost not only of the particular case that eventually reaches that agenda but also of the various lower court cases that create the legal conditions which encourage the high court to resolve the issue. The U.S. Supreme Court, for instance, prefers to decide only those issues that have "percolated" for some time in lower courts or that are the subject of conflicting rulings in the lower courts (Perry 1991, 230–4, 246–52). To reach the Court's agenda, then, a case generally must be representative (or at least a part) of a much larger and not ephemeral body of litigation. Thus, those who hope to place a particular issue on the Court's agenda must have sufficient financial resources to support extensive litigation in lower courts or must be able to rely on the resources of a broader class of litigants.

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[1] Lowell 1888 (and Kammen 1986) criticized the machine metaphor, arguing that the U.S. Constitution is far less machine-like than it has appeared to some observers.

Issues are not equally endowed with support, and so the resource prerequisite likely affects the nature of the judicial agenda. Access to resources is likely to be especially important for shaping the application of constitutional rights, because the monetary stakes for the affected parties typically are relatively low. Thus, Linda Brown, the named party in *Brown* v. *Board of Education* (1954), could never hope to recoup from a court victory the funds necessary to cover her legal expenses. Therefore, decisions to mobilize the law on public law rights plausibly depend to a great extent on access to resources. For these reasons, we should expect that the presence of civil liberties and rights on the agendas of supreme courts will depend on support beyond that provided by the immediate plaintiff in the case.

Three types of resources—organized group support, financing, and the structure of the legal profession—appear to be important conditions shaping access to the judiciary. Together these resources constitute the support structure for legal mobilization. First, a wide range of scholarship identifies organized group litigants as important influences on judicial agendas. Galanter (1974) suggests that "repeat players," typically organizations, fare significantly better in court and influence legal change and agenda setting significantly more than do "one-shot" litigants (that hypothesis has gained support in a range of research; see, e.g., Atkins 1991, Caldeira and Wright 1988, Epstein and Kobylka 1992, McCormick 1993, Songer and Sheehan 1992; but see Epstein and Rowland 1991; Sheehan, Mishler, and Songer 1992).

In recent decades the number and diversity of organized groups providing support for rights litigation have grown significantly in many countries (Epp 1995). In the United States, organized support for civil liberties and civil rights grew after about 1910 with the development of numerous rights-advocacy organizations, which contributed significantly to the judicial rights agenda. The NAACP Legal Defense Fund, for example, organized, financed, and provided legal counsel for many of the most important civil rights cases to reach the U.S. Supreme Court (Kluger 1977, Vose 1959, Wasby 1995). Similar organizations supported important Supreme Court cases in other areas of law, among them the development of fundamental constitutional rights in general (Walker 1990) and, in particular, the First Amendment's religion clauses (Sorauf 1976) and women's rights (O'Connor 1980).

Another factor contributing to access to the higher judiciary is financing, particularly from governmental sources. Governmental financing appears to be a necessary condition for the presence on the agenda of claims by the poor (Lawrence 1990) and criminal defendants. In the United States, legal aid in civil cases and the most important forms of aid for criminal defendants are relatively new developments within the last sixty years.

Finally, access to lawyers and the structure of the legal profession influence access to the judicial agenda. With some exceptions, the assistance of an attorney is necessary to take a case to a supreme court. Lawyers contribute to legal strategy and provide much of the network through which information about rights litigation travels (Kagan 1993, McGuire 1993). In addition, the degree of diversity in the legal profession appears to influence access to the judicial agenda. The more racially and ethnically diverse and open to women is a legal profession, the more likely it is to provide access to the courts to women and members of racial and ethnic minorities (see, e.g., Abel and Lewis 1988). In some countries, and increasingly in the last few decades, the legal profession has become ethnically diverse and contains a growing number of women. The structure of practice in the legal profession also appears to influence access to the judicial agenda. Law firms gain the benefits of scale economies, which they may use to support litigation campaigns that are not immediately financially productive. Conducting legal practice within firms, however, is a relatively recent development, beginning in the United States in the last hundred years (Galanter and Palay 1991) and spreading to some other countries after the early 1970s (Abel and Lewis 1988).

If the existence of a support structure is necessary for rights advocates to have access to the judicial agenda, we should expect developments in the support structure to be matters of political strategy and controversy. Indeed, this is the case. Much of the support structure's development in the United States and, as I shall discuss shortly, in Canada reflects the political strategies of liberals and egalitarians to use the courts for political change. Recently, conservatives in both countries have responded by developing competing legal advocacy organizations and by attempting to cut governmental funding for legal services. These controversies reflect a growing recognition that the development of law in general, and of rights in particular, is shaped by the nature and extent of the support structure.

Judicial Attitudes

The application of a bill of rights is likely to be influenced also by the policy preferences of judges. It is widely recognized that U.S. Supreme Court justices greatly vary in their votes on civil rights and civil liberties cases (Segal and Spaeth 1993). Moreover, justices vote strategically in setting their agenda, preferring those cases in which their position is likely to prevail (Brenner and Krol 1989, Perry 1991, 198–215). Thus, the extent of agenda space devoted to rights claims, and the nature of those claims, is conditioned by judicial attitudes. For instance, the U.S. Supreme Court's agenda shifted toward civil liberties and civil rights after the 1930s as judicial liberals gained dominance on the Court (Pacelle 1991). Based on the U.S. experience, we may expect that the extent of liberal control of a court will influence directly its agenda on civil liberties and civil rights.[2] Indeed, the importance of judicial attitudes may be so great as to influence the judicial agenda entirely independently of the presence or absence of a bill of rights.

Discretionary Docket

Although a court's attitudinal composition likely influences its agenda, that influence appears to be conditioned by the extent of discretionary control that judges have over their docket. There is substantial evidence that discretionary docket control contributes to the expansion of a public law agenda. Since many ordinary courts are required to decide nearly any case brought to them, their agenda consists mostly of private economic disputes. The U.S. Supreme Court, by contrast, gained nearly complete discretionary control over its agenda in 1925 as part of a reform aimed at clearing a range of routine private disputes from the Court's docket. As a consequence, the Court's agenda has changed profoundly, from a focus on ordinary economic disputes (contracts, torts, and the like) to a focus on public law (Casper and Posner 1976, Frankfurter and Landis 1927). Similarly, as state supreme courts in the United States gained discretionary control over their docket, their agenda has shifted away from private economic disputes and toward public law (Atkins and Glick 1976, Kagan et al. 1977). The correlation between a discretionary docket and a public law agenda has proven so common in the United States that we are likely to find it as well in Canada.

STUDY DESIGN AND DATA

Canada presents a nearly ideal test of these hypotheses. The Canadian Supreme Court is an active participant in what Tate and Vallinder (1995, 5) have called "the global expansion of judicial power," and there is little doubt that the Court's agenda has been transformed toward a focus on individual rights (Morton, Russell, and Withey 1989; Morton, Russell, and Riddell 1995). Most previous research, however, has not explicitly tested alternative explanations for that transformation but instead has assumed that it resulted primarily from adoption of the Charter of Rights and Freedoms in 1982 (but see Morton 1993, Morton and Knopff 1993). This study makes use of fortunate characteristics of Canada's recent history to conduct such a test. As I shall discuss in more detail shortly, Canada's support structure for legal mobilization began to grow in the late 1960s and continued to strengthen through the 1980s; in 1975 the Supreme Court gained nearly complete discretionary control over its agenda; in 1982, the country adopted a bill of rights; and in the mid-1980s judicial liberals gained a majority on the Court. The staggered timing of these developments makes possible an assessment of the alternative hypotheses.

I rely on data from a variety of sources. Information on the Court's judicial agenda was gathered from the official *Supreme Court Reports* in five-year intervals beginning in 1960 and ending in 1990. The sample includes all full decisions of one page or more reported in the annual volumes. For data on changes in the support structure for legal mobilization, I rely on a variety of primary and secondary sources, as cited in the following pages. For data on judicial attitudes, I rely on a variety of secondary sources, primarily the results of previous studies, as cited in the following pages.

The Charter of Rights and Freedoms

Canada adopted a constitutional bill of rights, the Charter of Rights and Freedoms, in 1982, and Canadians commonly attribute enormous symbolic and practical importance to the document. Its passage culminated a long and arduous battle by some Canadian politicians and activists to establish a working bill of rights. In the 1950s, in the absence of a bill of rights, the Canadian Supreme Court handed down a number of decisions on civil liberties that are generally regarded as landmarks (Snell and Vaughan 1985, 206–8). The Diefenbaker government passed a statutory Bill of Rights in 1960 in an attempt to consolidate and extend the Court's growing civil liberties jurisprudence, but the judicial conservatives who dominated the Court until the mid-1980s refused to give it effect. Meanwhile, Pierre Trudeau, then justice minister and later prime minister in the late 1960s and 1970s, proposed a constitutional bill of rights as part of a larger response to the growing threat of Quebec separatism. Trudeau apparently hoped to accomplish several purposes, among them protecting English speakers from French-only laws in Quebec, assuaging the fears of French speakers in other provinces, and, as Knopff and Morton (1985) have shown, encouraging the development of rights-based cleavages that would unite some Canadians across provincial boundaries. After political maneuvering that is too complicated to relate here (see Romanow, Whyte, and Leeson 1984; Banting and Simeon 1983), the government succeeded in 1982 in passing a greatly revised Charter and a new constitutional amending formula. The final list of rights resulting from that historic struggle is about seven pages long and contains detailed language inserted to discourage the courts from adopting narrow interpretations of its guarantees.[3] Significantly, the Charter's provisions formally authorize judicial review in cases brought by individuals.

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[2] The direct effect of judicial attitudes is likely to be qualified when conservative judges gain control of a court after a period of liberal dominance. In such a circumstance, the judicial conservatives are likely to continue deciding cases involving liberal rights claims but for the purpose of rejecting those claims. For instance, the Burger Court, which was conservative on criminal procedure, nonetheless maintained the Warren Court's high level of attention to criminal procedure precisely in order to reverse the policy direction of the law.

[3] The charter is more detailed than the U.S. Bill of Rights. The Charter contains separate lists of rights under the headings "Funda-

Both critics and supporters of the Charter claim that it has produced a number of important changes in Canadian politics and society. The Court's current chief justice has called passage of the Charter "a revolution on the scale of the introduction of the metric system, the great medical discoveries of Louis Pasteur, and the invention of penicillin and the laser" (quoted in Bogart 1994, 257). Political scientists have made more realistic but still sweeping claims about the Charter's effects. Knopff and Morton (1992, 1), for example, wrote that the Charter "is the occasion not just for new kinds of lawsuits, but also for a new form of constitutional politics. . . . In a myriad of ways, the Charter of Rights and Freedoms has truly transformed the Canadian political landscape since its enactment in 1982" (see also Cairns 1991, 1992).

The Charter is widely thought to have transformed, in particular, the Court's agenda, decisions, and workload. Most important, scholars argue that the Charter has increased the Court's level of attention to rights claims, its support for rights claims, its reliance on constitutional reasoning, and its exercise of judicial review (see, e.g., Knopff and Morton 1992, Bogart 1994). In addition, several scholars have argued that the Charter encourages interest groups to proliferate and to take their demands increasingly to the Supreme Court, thereby promoting growing complexity of conflict in cases (Bogart 1994, 301–3; Knopff and Morton 1992, 26–9; Morton 1987).

From these observations, I have derived several indicators of the Court's agenda that may be expected to reveal the Charter's influence especially clearly. They are: (1) the proportion of the issue agenda devoted to civil rights and civil liberties; (2) the frequency of exercise of judicial review; (3) the level of support for civil rights and liberties; (4) the extent of reliance on constitutional foundations for decisions, which I expand to include all "higher law" foundations, namely, the common law standard of natural justice, the 1960 statutory Bill of Rights, constitutional law other than the Charter, and the Charter; (5) the extent of participation in cases by interest groups and other third parties; and (6) the size of the docket.

Although Canadian observers nearly universally attribute profound effects to the Charter, Morton (1993) and Morton and Knopff (1993) recently have argued that the effects often attributed to the Charter alone are, instead, the result of active political pressure by the "Court Party," an informal coalition of rights-advocacy groups, lawyers, and judges. As Morton states, "the *Charter* itself is not so much the cause of the revolution as the means through which it is carried out" (1993, 181). That analysis is consistent with the support structure hypothesis.

Canada's Support Structure for Legal Mobilization

The Canadian support structure for legal mobilization grew dramatically between 1965 and 1990, with much of the growth preceding adoption of the Charter. I briefly survey developments in the three components of that structure.

Rights Advocacy Organizations. The development of private rights-advocacy organizations in Canada occurred in a relatively short time, roughly between the late 1960s and the early 1980s. Prior to 1970, business and agricultural groups dominated the Canadian interest group system (Presthus 1974), but thereafter the number of nonproducer advocacy organizations virtually exploded (Paltiel 1982, Pross 1975). Interest groups focusing on civil liberties and civil rights, in particular, did not exist before the mid-1960s, but their prominence in Canadian politics had grown substantially by the early 1980s. The two principal ones, the British Columbia Civil Liberties Association and the Canadian Civil Liberties Association, were founded in 1962 and 1964, respectively, but only became relatively active after about 1970. In the 1970s a number of regional civil liberties organizations also formed (Epp 1995, 260).

Advocacy groups supporting civil rights also began to form after the mid-1960s. In the area of women's rights, the National Action Committee on the Status of Women was formed in 1971; the Canadian Advisory Council on the Status of Women, a quasi-state organization, in 1973; the National Association of Women and the Law in 1977; and the Women's Legal Education and Action Fund in 1985. The major aboriginal rights organizations also formed in the late 1960s and early 1970s. Of 239 major aboriginal political organizations existing in 1993, 43 formed in the 1960s, 92 in the 1970s, and 28 in the 1980s (Frideres 1993, 288). The Advocacy Resource Centre for the Handicapped, a leading organization advocating expanded rights for the handicapped, was formed in 1980.

After the mid-1960s the national and provincial governments created agencies that have acted as advocates of the new civil rights and liberties. The provinces and the national government also began adopting comprehensive human rights codes prohibiting private discrimination on a broad range of grounds; along with these codes, they created quasi-judicial human rights commissions to hear discrimination claims, from which there are appeals to the regular courts (Knopff 1990, 36–40; Tarnopolsky 1982, 25–37, 434–9). In addition, in the late 1960s several provinces and the federal government created law reform commissions that provide continuing advice to legislatures on legal reform. Both types of

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mental Freedoms" (similar to the liberties contained in the U.S. First Amendment); "Democratic Rights" (the right to vote and limitations on the time in which the House of Commons may remain in authority without facing reelection); "Mobility Rights" (the right of citizens to leave and enter the country and to move and live in any province); "Legal Rights" (procedural due process); "Equality Rights" (equality before the law and equal protection of the law without regard to race, national or ethnic origin, color, religion, sex, age, or mental or physical disability); and "Official Languages of Canada" and "Minority Language Educational Rights" (providing for the equal status of French and English). The Charter also guarantees the right to redress through the judicial system, states that the Constitution is the supreme law of Canada, and declares that any laws inconsistent with it are "of no force and effect."

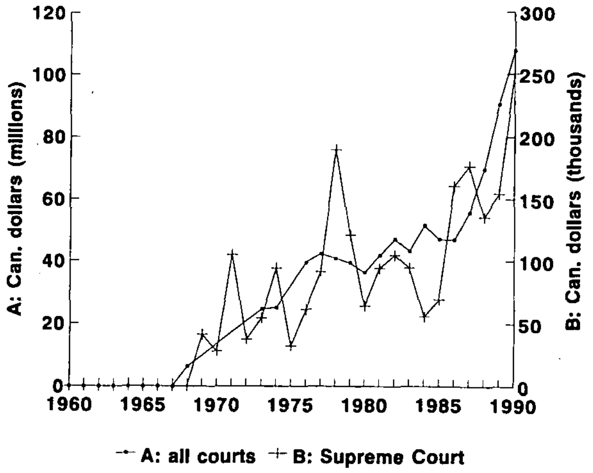
commissions became institutional sites for liberal rights advocacy, and there has been a fluid interchange of talent and legal resources among these governmental agencies, the law schools, and private rights advocacy organizations (Devins 1993; Morton 1993, 193–4; Morton and Knopff 1993, 72).

Canadian rights advocacy organizations are primarily liberal or left-liberal in orientation. To a significant extent this reflects deliberate policies by the national government to cultivate liberal advocacy organizations. The Canadian Department of the Secretary of State, under the direction of Trudeau's Liberal Government in the late 1960s, developed an aggressive program to finance citizens' advocacy organizations focusing on such issues as women's rights, language rights, and multiculturalism (Pal 1993). Nonetheless, several conservative organizations support litigation, among them the National Citizen's Coalition and REAL Women, but they have enjoyed much less success than liberal groups because, as Morton and Knopff (1993, 70) observe, "they are decidedly swimming against the ideological tide."

Government Sources of Financing. The national and provincial governments have developed a variety of programs that finance rights litigation and advocacy. In the decade after 1965, in a major policy revolution, the Canadian provinces created legal aid programs that finance both civil and criminal cases. The rapidity of the development cannot be overstated. Ontario incorporated an official legal aid program in 1966; by the end of 1975 all the provinces had created such a program (National Legal Aid Research Centre 1981). The federal government contributed to the rapid growth with subsidizes (Zemans 1978). In brief, the provinces and the national government created virtually the entire legal aid apparatus in less than a decade.

Spending on court cases by the new legal aid organizations increased dramatically in the 1970s. The level of funding by the Ontario Legal Aid Program, the largest of the provincial programs and the first to be created, illustrates that growth (see Figure 1). The level of funding grew between 1970 and 1977, and then rose rapidly again in the late 1980s.[4]

FIGURE 1. Legal Aid Expenditures (Ontario)



Source: Ontario Legal Aid Plan Annual Reports, adjusted for inflation.

The Court Challenges Program, a set of funds created specifically for financing test cases, is another government-sponsored legal program that began prior to passage of the Charter. The program was established in 1978 to finance court cases on language rights protected under the Constitution Act of 1867; since then, it has "supported almost every major language law case at the [Supreme] Court" (Brodie 1992). The government added a component to the program in 1985 to finance cases under the equality provisions in the Charter. Between 1985 and 1992, when the program was eliminated by the Mulroney government, the equality component financed 178 court cases at all levels of the system, including 24 cases in the Supreme Court (Court Challenges Program 1992). Trends in the program's funding and case support across time are not easily identified, although it is clear that the language component supported some cases prior to passage of the Charter (Court Challenges Program 1992). It is also clear, however, that the creation of the equality component in 1985 resulted directly from the desire of interest groups and government officials to implement the equality rights in the Charter (Brodie 1992). Conservative groups have criticized the program for supporting only liberal or left-liberal causes, and the government briefly eliminated the program's financing from 1992 to 1994.

The national government's Department of Indian Affairs and Northern Development (DIAND) also began sponsoring test case litigation prior to adoption of the Charter. DIAND began funding court cases in 1965 but significantly increased its support in the late 1970s, financing about 25 test cases before 1982 (Milligan 1989, 16; Oscapella 1988). DIAND has provided financial support for most of the aboriginal rights cases that have reached the Court (author's interviews with lawyers for native rights organizations, June 1992).

The Legal Profession. Several highly significant changes in the Canadian legal profession have occurred since 1945, with the most important developments between the mid-1960s and the early 1980s. First, Canada's system of legal education changed dramatically as the importance and autonomy of law schools increased. In Canada, as in the United States, training for the practice of law has shifted from apprenticeship, a system in which legal education is dominated by the relatively conservative interests of the practicing bar, to law schools. Ten of Canada's 20 law schools were created after World War II, and the number of full-time law professors almost doubled between 1971 and 1982 alone (Stager and Arthurs 1990, 90, 302–3). As the importance and autonomy of the law schools grew, legal training increasingly emphasized theoretical and constitutional issues (Bushnell 1992, 281–2, 341–2, 343–6; Stager and Arthurs 1990, 84–91). By 1982, the year of the Charter's adoption,

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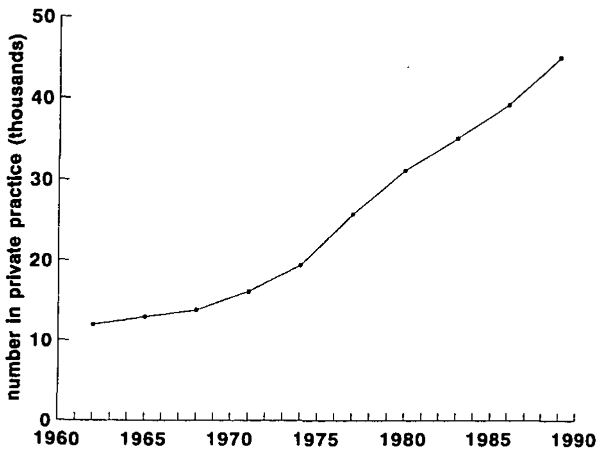
[4] Some of the growth in legal aid in the late 1980s may have been due to the Supreme Court's decision in *Singh* (1985), which required the government to conduct hearings before deporting illegal immigrants and to ensure that immigrants have counsel in such hearings. I am indebted to Ian Brodie for this observation.

FIGURE 2. Canada's Lawyer Population



Source: Stager and Arthurs (1990, 146).

Canadian law professors were remarkably young (the median age was 38) and generally supportive of a growing policymaking role for the judiciary on civil liberties and civil rights (Stager and Arthurs 1990, 303). For instance, in a survey conducted in 1982–83, 18% of law professors reported working for a law reform commission, and 25% reported working for a public interest or community group (Stager and Arthurs 1990, 304).

The growth of law schools also provided an institutional base for critical scholarship and advocacy on constitutional issues. Bora Laskin, chief justice of the Supreme Court from 1973 to 1984, led the push in the 1950s and 1960s for scholarly critique and advocacy. In 1951 he attributed the Court's conservatism to "the conservative tradition of the Canadian legal profession . . . [and] the late development of university law schools" (Laskin 1951, 1046). By the 1980s, by contrast, many in the growing ranks of law school professors actively pursued advocacy scholarship favoring liberal judicial interpretation of civil rights and liberties (Morton and Knopff 1993, 61, 72–5).

In addition, in the 1970s Canada's lawyer population grew dramatically and began to diversify, and attorneys increasingly engaged in advocacy. The most significant rise in the number of lawyers occurred between 1971 and 1981, when their ranks more than doubled (see Figure 2) (Stager and Arthurs 1990, 149). As Figure 2 illustrates, the attorney population grew at a faster pace prior to 1981 than afterward; thus, the Charter induced no unprecedented growth in the number of lawyers.

The Canadian legal profession also began to diversify by ethnic origin and sex after 1970. In 1961, 79.6% of all Canadian lawyers were from either a British or French background; by 1971, that proportion had declined to 74%; by 1981 it had dropped to 68.1% (Stager and Arthurs 1990, 148–54). The proportion of British origin, in particular, declined from 56.3% in 1961 to 44.5% in 1981. Ethnic minorities, who constituted only about one-fifth of the total in 1961, accounted for almost one-third by 1981. The rate of growth in the representation of women in the legal profession was even more dramatic. In 1961, only 3% of all lawyers were female; their proportion grew to 5% in 1971, 15% in 1981, and 22% in 1986 (Stager and Arthurs 1990, 148–50).

The structure of legal practice also began to change in the 1970s as the number and size of large law firms began to grow. Both the quantity of such firms and the lawyers working in them have risen at a significantly faster pace than the number of lawyers in general (Stager and Arthurs 1990, 170–7).

Taken together, these changes in Canada's interest group system, governmental financing of litigation, and the legal profession fundamentally transformed the Canadian support structure for legal mobilization *prior to adoption of the Charter in 1982.*

The Canadian Supreme Court: Attitudinal Composition and Docket Control

Due to patterns in the Supreme Court's attitudinal composition from 1960 to 1990, the Canadian case also makes possible a test of the judicial attitude hypothesis. Recent research has confirmed that judges in Canada, like their counterparts in the United States, differ significantly in their voting on rights cases (Heard 1991; Morton, Russell, and Riddell 1995; Tate and Sittiwong 1989). Nonetheless, what is striking about the Court's attitudinal composition is the unambiguous dominance by judicial conservatives over the entire period prior to the mid-1980s. Judicial liberals gained a majority on the Court only after 1985. Table 1 illustrates the changing composition of the Court by characterizing the justices as liberal (+) or conservative (−) in their attitudes toward the new rights claims. Prior to 1982 there were three leading proponents of an expanded civil liberties agenda on the Supreme Court—Ivan Rand (served 1943–59), Emmett Hall (1962–73), and Bora Laskin (1970–73 as associate justice, 1973–84 as chief justice). Nonetheless, no majority of justices consistently supported the expansion of judicial policymaking on civil liberties and civil rights until the late 1980s.

Canada also presents an ideal test of the docket-control hypothesis. The Court's workload began growing in the early 1970s, prompting Parliament to grant the Court in 1975 nearly complete discretion over which cases to decide (Bushnell 1982; Knopff and Morton 1985). There remain appeals as of right principally in some criminal cases, but the vast majority of cases coming to the Court are now on the discretionary docket. Thus, 1975 marked a significant turning point in the Canadian Supreme Court's institutional history.

PATTERNS IN THE CANADIAN SUPREME COURT'S AGENDA

In light of the various changes in Canada's Constitution, its support structure for legal mobilization, the Supreme Court's attitudinal composition, and the Court's docket control, what changes have occurred in the Court's agenda, and when did they begin? I pursue here a data triangulation strategy, using a variety of alternative measures of the Court's agenda and workload from 1960 through 1990. The data generally tell much the same

TABLE 1. Attitudinal Composition of the Canadian Supreme Court

1960	1965	1970	1975	1980	1985	1990
−*Kerwin*	−*Taschereau*	−*Fauteaux*	+*Laskin*	+*Laskin*	+*Dickson*	+*Dickson*/+*Lamer*
−Abbott	−Abbott	−Abbott	−Beetz	−Beetz	−Beetz	−Cory
+Cartwright[a]	+Cartwright	+Hall	+Dickson	−Chouinard	−Chouinard	−Gonthier
−Fauteaux	−Fauteaux	−Judson	−Grandpre	+Dickson	−Estey	−La Forest
−Judson	+Hall	+Laskin	−Judson	−Estey[b]	−LaForest	+/−L'Heureux-Dubé[c]
+Locke	−Judson	−Martland	−Martland	+Lamer	+Lamer	+McLachlin
−Martland	−Martland	−Pigeon	−Pigeon	−Martland	−Le Dain	+/−Sopinka[d]
−Ritchie	−Ritchie	−Ritchie	−Ritchie	−McIntyre	−McIntyre	−Stevenson
	+Spence	+Spence	+Spence	−Pigeon	+Wilson	+Wilson
				−Ritchie		

Sources: Bushnell 1992; Heard 1991; Morton, Russell, and Riddell 1995; Tate and Sittiwong 1989.
Note: chief justices are italicized at the top of each column
(+) indicates liberalism on civil liberties and rights
(−) indicates conservatism on civil liberties and rights

[a] Bushnell (1992, 317) places Cartwright in the group of justices who were opposed to the use of fundamental rights in judicial review. That characterization differs significantly from Cartwright's high civil liberties score in research by Tate and Sittiwong (1989). Following Tate and Sittiwong, Cartwright is categorized here as a liberal because his votes overwhelmingly supported the civil liberties position in disputed cases.

[b] According to Bushnell (1992, 487–8), Estey opposed the growing rights-based activism of the Court after 1985, publicly stating so upon resigning in 1988. This differs from his above-average civil liberties support score as reported by Tate and Sittiwong (1989).

[c] Morton, Russell, and Riddell (1995) report that L'Heureux-Dubé tends to support Charter claims on equality rights but tends to oppose Charter claims on legal rights (of the criminally accused).

[d] Morton, Russell, and Riddell (1995) report that Sopinka tends to support Charter claims on legal rights (of the criminally accused) but tends to oppose Charter claims on equality.

story: Significant changes in the Court's agenda, on a number of dimensions, began in the early 1970s and continued at rates that remained largely the same at the time the Charter was adopted in 1982. There are some exceptions to that general pattern. Most important, both the Court's level of support for rights claims and the number of requests for the exercise of judicial review apparently rose in response to passage of the Charter, but on most dimensions the agenda transformation began at least ten years before. In this section I survey these developments, leaving a discussion of their significance to the following section.

The Issue Agenda

First, the Court's issue agenda changed dramatically between 1960 and 1990 (see Figure 3). The proportion of the docket devoted to civil liberties and civil rights grew dramatically, and the proportion devoted to tax cases and ordinary economic disputes declined dramatically.[5] Both developments began prior to adoption of the Charter in 1982. Civil rights and civil liberties cases constituted 13% or less of the Court's agenda before 1975, and by 1990 they claimed about 60%—but the growth rate between 1980 and 1985 (86%) was only marginally faster than between 1975 and 1980 (78%). Similarly, the proportion of the agenda devoted to tax and ordinary economic issues abruptly began to decline after 1975 but prior to adoption of the Charter in 1982.[6]

Judicial Review

The Court's exercise of judicial review increased between 1960 and 1990, but the growth appears unaffected by passage of the Charter. Figure 4 presents two summary measures, the number of cases in which the Court considered whether to overturn a law, and the number of cases in which it did so. The use of judicial review grew moderately between 1960 and 1990, but the largest increase occurred between 1975, when the Court struck down a law in only one case, and 1980, when it struck down laws in five cases. After 1980, although the number of laws struck down increased, the rate of growth appears to have declined.[7] Yet, the number of cases in which litigants asked the Court to exercise judicial review significantly rose after passage of the Charter. Although the growth apparently originated in the late 1960s or early 1970s, between 1985 and 1990 the number of requests for judicial review increased dramatically. In 1985 the Court decided eight cases centering on a request for the exercise of judicial review; in 1990 it decided almost thirty such cases.

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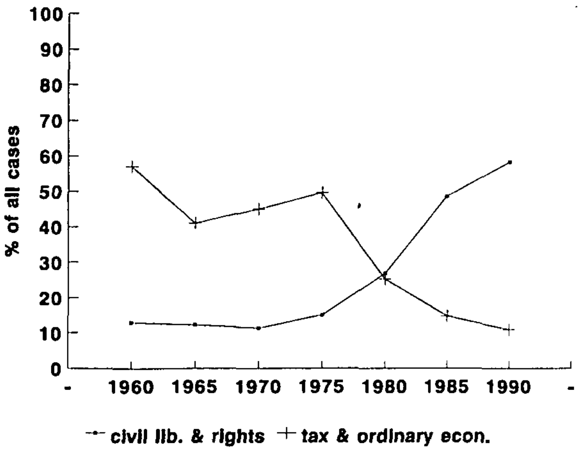
[5] In using these measures, I follow Pacelle (1991). I include in the rights agenda, in addition to constitutional rights, some statutory or common law rights that have taken on a status analogous to constitutional rights. They include the common law concept of "natural justice" and statutory prohibitions of discrimination. The proportion of the agenda devoted to rights that I report here is somewhat higher than that reported by Morton, Russell, and Riddell (1995), probably because my analysis is not limited to Charter cases and because I use, as a base, only full decisions (one page or more of opinion). Ordinary economic issues, as defined here, include all tax disputes (unless they raise a rights claim) and a range of ordinary private disputes, among them contracts, torts, wills and estates, and the like.

[6] There is also some evidence that the Court's ordinary economic cases are themselves shifting toward a focus on rights. For example, in *Norberg* v. *Wynrib* (1992), a tort case, the decision centered on the issue of sex discrimination. Thus, the trends presented in Figure 2 likely understate the extent of the transformation from ordinary economic issues to rights issues. I am indebted to Ian Brodie for this observation.

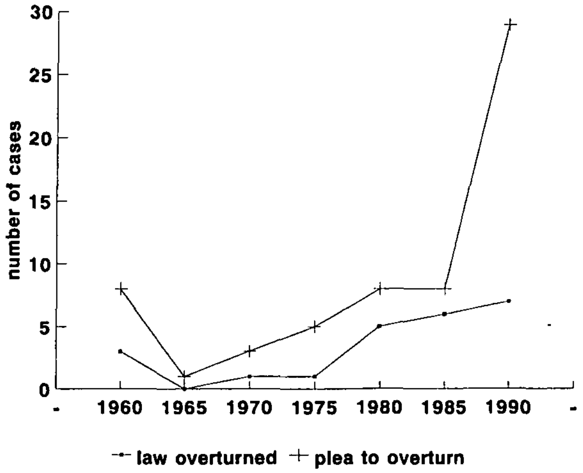
[7] At its high point in 1990, the Court overturned fewer than ten federal and provincial laws. Compared to the average of about 16 state laws and just under two federal laws struck down by the U.S. Supreme Court per year in the 1980s (Baum 1992, 188–90), that is not a particularly high number. There are far fewer provinces than states, however, to generate potential conflict with Supreme Court policy, and so we might expect the use of judicial review to be somewhat less frequent in Canada than in the United States.

FIGURE 3. Issue Agenda of the Canadian Supreme Court



Source: Author's coding of Supreme Court decisions.
N = 1960, 77; 1965, 73; 1970, 80; 1975, 119; 1980, 119; 1985, 68; 1990, 110.

FIGURE 4. Judicial Review by the Canadian Supreme Court



Source: Author's coding of Supreme Court decisions.

Support for Rights Claims

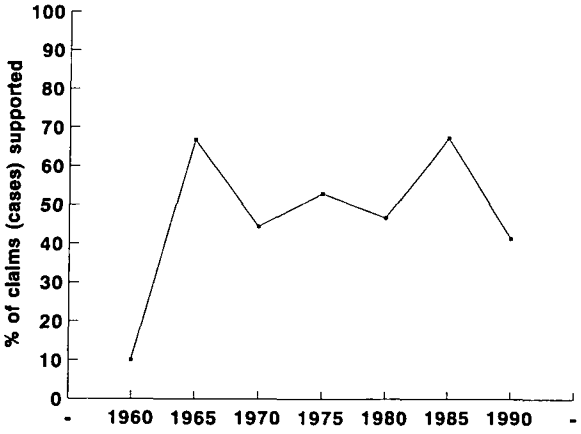
Adoption of the Charter may have affected the Court's level of support for rights claims, but, if so, the effect was neither clear nor dramatic (see Figure 5).[8] In the early period of the study, changes in the level of support are an artifact of the small number of rights cases, and so most of our attention should focus on the years after 1970. At the aggregate level, the Court's support for rights claims grew significantly between 1980 and 1985 but dropped again by 1990 (this observed decline is consistent with results of research by Morton, Russell, and Riddell 1995). Contrary to expectations, the Charter had no sustained effect on the Court's level of support for the rights claims on its agenda. These aggregate results, however, do not control for changes over time in the nature of the rights claims being decided. In addition, the aggregate results do not reveal a growing dispute on the Court in the late 1980s over which rights claims should be supported: in broad terms, some justices favored criminal due process and negative liberties, while others favored egalitarian claims (Morton, Russell, and Riddell 1995, 43–9). Nonetheless, the results in Figure 5 suggest that the Charter's effects have been more subtle than is generally believed.

Basis for Decisions

Trends in several indicators of the basis for Court decisions reveal another dimension of the Court's agenda. First, even after passage of the Charter, the proportion of cases involving rights claims *not* founded on that document continued to expand (see Figure 6). There was a substantial increase in non-Charter rights cases in 1985 over 1980; in fact, a surprisingly small proportion of the cases in 1985 were decided on Charter grounds, probably in part because of the time lag in bringing Charter cases through several levels of the judicial system. The drop in the number of non-Charter cases in 1990 resulted either from Charter cases squeezing non-Charter cases off the agenda or from a translation of what previously would have been non-Charter cases into Charter language. These data cannot answer that question. But it is clear that Charter cases themselves were only a small proportion of rights claims in 1985 and that, for much of the period before the late 1980s, rights claims developed in formal independence of the Charter.

Second, trends in the Court's use of all higher law foundations—including non-Charter constitutional law (primarily federalism), the 1960 Bill of Rights, the common law standard of natural justice, and of course,

FIGURE 5. Support for Rights Claims by the Canadian Supreme Court



Source: Author's coding of Supreme Court decisions.
N = 1960, 10; 1965, 9; 1970, 9; 1975, 17; 1980, 30; 1985, 31; 1990, 60.

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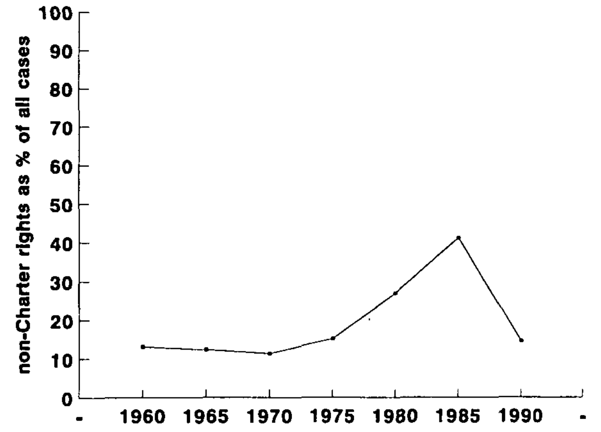
[8] I define "support" as decisions that the rights claimant would have regarded as favorable. I omitted from this coding decisions in which the outcome was ambiguous or split, as well as cases in which there were competing rights claims.

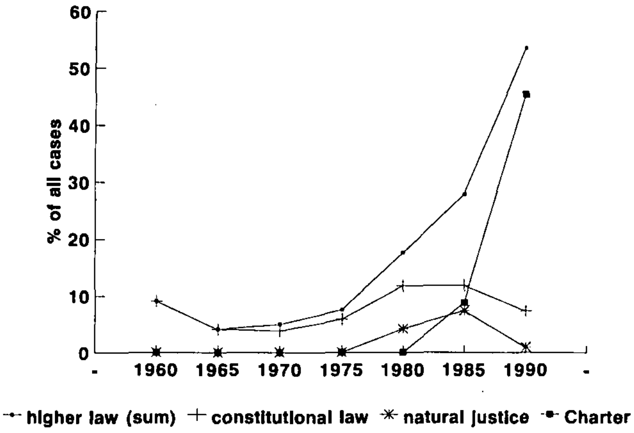
FIGURE 6. Non-Charter Rights Cases in the Canadian Supreme Court



Source: Author's coding of Supreme Court decisions.
N = 1960, 77; 1965, 73; 1970, 80; 1975, 119; 1980, 119; 1985, 68; 1990, 110.

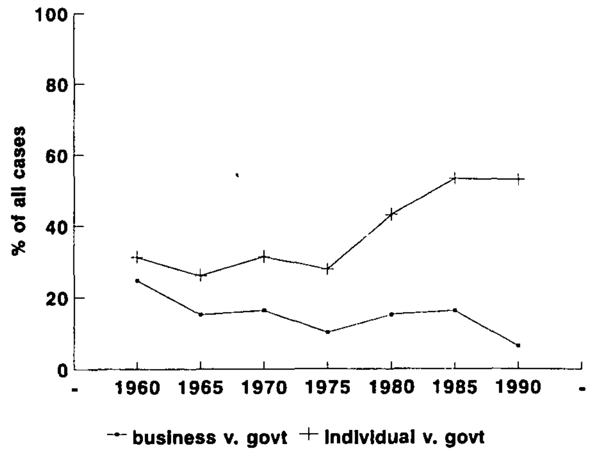
the Charter after 1982—are presented in Figure 7. The proportion of cases decided on all higher law grounds began to rise prior to 1982. Part of that growth reflects an increase in the number of federalism cases decided by the Court (Monahan 1987, 18–21; Russell 1985). After 1982, the proportion of cases in each of the non-Charter categories declined as the proportion of cases involving the Charter increased. By 1990, Charter cases began to replace non-Charter cases on the agenda, although even in 1990 a significant portion of higher law cases was not based on the Charter. Because the use of higher law foundations for decisions began to increase as early as 1975 and because the most important changes after 1982 consisted not of an acceleration in the rate of growth for *all* higher law cases but a shift toward Charter foundations away from other higher law foundations, it is possible that passage of the Charter simply changed the foundation on which claimants based their challenges.

FIGURE 7. Higher Law Foundations for Decisions in the Canadian Supreme Court



Source: Author's coding of Supreme Court decisions.
N = 1960, 77; 1965, 73; 1970, 80; 1975, 119; 1980, 119; 1985, 68; 1990, 110.

FIGURE 8. Individuals and Businesses v. Governmental Parties in the Canadian Supreme Court



Source: Author's coding of Supreme Court decisions.
N = 1960, 77; 1965, 73; 1970, 80; 1975, 119; 1980, 119; 1985, 68; 1990, 110.

Types of Litigants

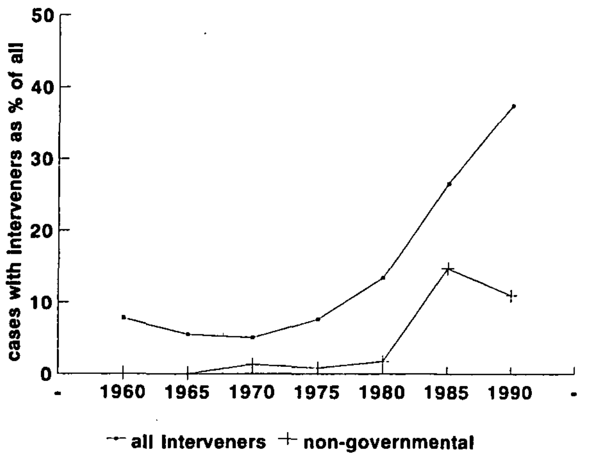
The nature of the parties appearing in Court cases also changed between 1960 and 1990. A useful indicator consists of trends in the types of private litigants who oppose government actors. I focus here on individuals, who often must rely on external sources of support to reach the Court, and on businesses. Over the period of the study, the presence of individuals increased, and the presence of businesses decreased (see Figure 8). Significantly, the growing presence of individual litigants began before passage of the Charter in 1982, and the rate of growth did not increase after passage.

The extent of intervention in Court cases by third parties also has grown since 1960 (see Figure 9). Third-party intervention in the Canadian judiciary is analogous to amicus curiae participation in the U.S. courts, and is often taken as an indicator of the degree to which the Court has become politicized. Brodie (1995; see also 1992) found that third-party intervention began to increase before passage of the Charter, and my data also clearly indicate that trend. Some growth is apparent as early as 1975, and substantial changes are undeniable by 1980. As Brodie (1995) points out, however, third-party intervention by nongovernmental organizations began to increase only after 1980.

Caseload

The number of cases brought to the Court grew dramatically over the period studied, but almost all the growth occurred before 1982 (see Figure 10). The Court's docket is composed of two broad categories, leave to appeal applications (over which the justices have complete discretion) and appeals by right (which the Court

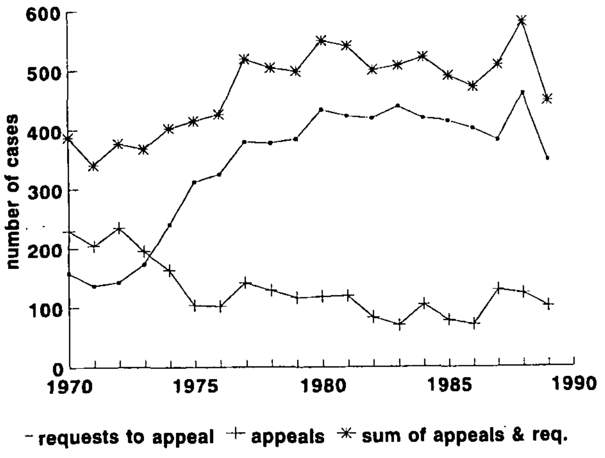
FIGURE 9. Third-Party Interveners in the Canadian Supreme Court



Source: Author's coding of Supreme Court decisions.
N = 1960, 77; 1965, 73; 1970, 80; 1975, 119; 1980, 119; 1985, 68; 1990, 110.

theoretically must hear). As noted earlier, a statutory change in the Court's jurisdiction in 1975 shifted much of its docket to the discretionary leave category, thus giving the Court greater discretion over which cases to decide. As a result, the number of leave applications filed per year began to grow around 1975, accompanied by a corresponding drop in the number of appeals. This drop, however, did not entirely compensate for increases in the number of leave applications, and so the total number of cases coming to the Court rose substantially between the mid-1970s and early 1980s. The period of significant growth ended in the early 1980s, prior to passage of the Charter. In short, litigants began turning to the Court in greater numbers long before passage of the Charter, and that event produced no new spurt in the number of cases brought to the Court.[9]

FIGURE 10. Caseload of the Canadian Supreme Court



Sources: Calculated from Bushnell (1982; 1986) and *Bulletin of the Supreme Court of Canada*.

DISCUSSION

The Canadian Supreme Court's agenda indeed has been transformed in the last several decades. The Court is now a major constitutional policymaker, focuses much of its attention on civil rights and liberties, increasingly decides cases brought by individuals who are supported by interest groups and government financing, increasingly faces complex disputes involving large numbers of parties, increasingly relies on higher law foundations for its decisions, increasingly entertains requests to strike down laws, and increasingly does strike down laws.

Most observers have attributed this broadly based transformation to Canada's recent adoption of the Charter of Rights and Freedoms or, secondarily, to the efforts of left-liberal justices on the Court. The evidence presented here, however, suggests that the Charter's influence is overrated and that judicial liberals gained control of the Court too late to have done more than encourage existing developments. Instead, changes in the Court's agenda appear to have resulted from the combined influence of two developments, the shift to a discretionary docket in 1975 and the development of a support structure for legal mobilization.

The shift to a largely discretionary docket in 1975 significantly contributed to the agenda transformation. When the Court gained control over its docket, it abruptly began moving private disputes from its agenda, making way for a growth in cases related to public law in general and to civil liberties and rights in particular. This shift occurred even though judicial liberals were in the minority on the Court at the time and remained so until after the mid-1980s. The experience of the Canadian Supreme Court in this respect closely parallels that of other courts, in particular the U.S. Supreme Court, that have gained discretion over their agenda. Given a choice, judges apparently prefer to decide rights cases rather than private economic disputes. Perhaps this is because rights cases are more interesting and expand judges' influence more than do ordinary private disputes, and perhaps because rights cases are difficult for judges to avoid.

The growth of the support structure for legal mobilization also contributed to the agenda transformation. The various components of the support structure began growing and diversifying just before the Court's agenda transformation started, and the support structure continued to grow as that transformation unfolded. Temporal priority and correlation alone cannot prove causation, but the weakness of the primary alternative explanations, those focusing on judicial attitudes and adoption of the charter, lends credibility to the support structure hypothesis.

Moreover, there is direct evidence of causal links between the support structure and the Court's agenda. Government financial aid has supported a range of important rights cases that have reached the Court. Legal aid, for instance, directly contributed to the growth of the Court's agenda on criminal procedure. In interviews conducted for this study, lawyers who were active in criminal defense and human rights work in the 1970s identified legal aid as the principal source of financing for criminal cases before the Court. For example, a leading criminal defense lawyer who has argued many cases before the Court said, "oh, it was legal aid, of course. None of those cases could have gotten to the Canadian Supreme Court without it" (interview with author, June 1992). Government financing also supported other types of rights cases. The Court Challenges Program funded virtually all the language rights cases to reach the Court (Brodie 1992) and many of the equality cases. Financing by DIAND supported nearly all the aboriginal rights cases to reach the Court.

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[9] The surprising lack of significant growth in the number of leave applications in the 1980s may have resulted from the Court's refusal to abandon oral hearings on leave to appeal applications, which placed time restrictions on the number of applications that could be heard.

In addition, both the diversification of the Canadian legal profession and the growth of large law firms influenced the Court's agenda.[10] In the 1980s, large Toronto firms allowed a number of their female attorneys to divert time and resources to the development of a legal strategy and funding for women's rights litigation (Razack 1991, 29–42). Those lawyers created the Women's Legal Education and Action Fund (LEAF) in 1985, which has financed virtually all the women's rights cases in the Court since 1985.

Even the Charter itself may be understood as a product of the changes in the Canadian support structure that transformed the Court's agenda. A number of rights advocacy organizations directly influenced the language of the Charter in highly significant ways by participating in the drafting process or by putting pressure on the national government's negotiators (Kome 1983, Morton 1993; Pal and Morton 1986). For instance, the Canadian Civil Liberties Association, along with several other groups, successfully lobbied for significant changes in the Charter's procedural rights provisions (Epp 1995, 297–9).

Although both the Charter and the Court's agenda transformation reflect changes in the support structure for legal mobilization, the Charter nonetheless has exerted an independent influence on some aspects of the Court's decision making. In particular, the Court's level of support for liberal rights claims grew, albeit temporarily, after passage of the Charter. Furthermore, this growing support apparently encouraged litigants to bring more such cases, as evidenced by the dramatic growth between 1985 and 1990 in the number of requests for judicial review. As Manfredi (1993, 170–2, 212–7) has argued, the Court's willingness to interpret the Charter's provisions broadly and creatively, and the willingness of interest groups to constitutionalize their claims through litigation, interact to heighten the policymaking role of the judiciary.

Finally, although the majority of Supreme Court justices did not actively encourage the agenda transformation documented here, some, particularly those identified as liberals in Table 1, certainly did. In addition, majorities of the Court made several decisions that significantly opened access to the Court's agenda. In several decisions in the late 1970s, the Court relaxed its rules on standing, allowing individuals and groups to pursue public interest lawsuits, and in the mid-1980s the Court opened its doors to third-party intervention by interest groups. These decisions surely contributed to the agenda transformation.

A number of questions remain to be explored. My data collection at five-year intervals has allowed a survey of only the broadest trends in the Canadian Supreme Court's agenda, and much still may be learned by examining year-by-year developments. In particular, there is evidence that the Charter encouraged at least some of the justices to lend greater support to rights claimants, but the nature and extent of that influence could be explored more adequately with research designed to isolate that relationship. For example, we would learn much about the Charter's influence by studying justices' votes and case outcomes in various categories of cases over the period of transition to the Charter, controlling for the justices participating in the decisions, fact patterns in the cases, and bases for the decisions. I suspect that such a research strategy would show that the adoption of the Charter indeed affected judicial decision making. But any such effect should be interpreted in the context of the broader trends reported here.

In addition, this study did not examine the cultural or educative effects of a bill of rights. Some might argue that the Charter created a culture of rights-claiming in Canada. I would speculate that this effect, too, depended on the strategic efforts of rights advocacy groups. Brodie and Nevitte (1993), for instance, have shown that a number of the cultural developments commonly attributed to the Charter in fact preceded its adoption and appear to reflect the development of new social movements in Canadian society. As Morton and Knopff (1993, 76) suggest, however, the Charter likely contributed to the strength of these social movements.

Finally, more research is needed on the relationship between support structures and the implementation of judicial decisions. Undoubtedly, courts lack powers of enforcement comparable to those of legislatures and executives. But there is evidence that organizational support for judicial decisions greatly influences the effect of those decisions (see, e.g., McCann 1994). Several questions remain unanswered, particularly whether the extent of implementation varies directly with the strength of the support structure underlying a particular legal right.

CONCLUSION

Sunstein has written that "rights do not fall from the sky" but depend on carefully crafted institutions and a rights-supportive culture (1995, 61). In recent decades constitutional engineers have relied heavily on bills of rights as a means for protecting rights. As James Madison recognized more than two centuries ago, however, bills of rights are not self-activating. Interpreting and developing the often ambiguous provisions of a bill of rights depends on mobilization of the law by individuals, but they typically lack the capacity to take cases to a country's highest court. The effects of a bill of rights, therefore, depend on the extent of organized support for mobilization of the law. Admittedly, a bill of rights may affect judges' willingness to strike down legislation or to check official action, but in the absence of adequate resources for legal mobilization, few noneconomic cases are likely to reach the judicial agenda, and judges will have few occasions to use their constitutional authority. Thus, constitutional reform alone, in the absence of resources in civil society for legal mobilization, is likely to produce only empty promises.

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[10] Although the diversification of the Canadian legal profession contributed to the support structure for legal mobilization, it is also clearly true that a number of leading litigators on rights issues have been from the dominant demographic base of the legal profession, white English Canadians (Razack 1991). The transformation of the legal profession, therefore, has been ideological as well as demographic.

To what extent may we generalize from these conclusions to the experience of other countries? If a bill of rights profoundly and independently influences politics in any country, those effects should be clearest and strongest in Canada. There, observers commonly attribute to the Charter a number of profound effects; moreover, the Charter intervened in an otherwise comparatively stable political system, and so its influence should be easily isolated. But even in Canada, an ideal case, some effects attributed to the Charter resulted instead from the growing capacity of rights claimants to pursue litigation, and other effects that indeed resulted from the Charter depended in addition on those changes in litigant capacity. Moreover, similar support structures appear to influence the judicial agenda in other countries, particularly in the common law world, where access to the higher courts depends on private initiative.

None of this indicates that bills of rights are irrelevant to politics. Bills of rights matter, but only if civil societies have the capacity to support and develop them.

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Partisan Effects of Voter Turnout in Senatorial and Gubernatorial Elections

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Conventional wisdom holds that higher turnout favors Democrats. Previous studies of this hypothesis rely on presidential and House elections or on survey data, but senatorial and gubernatorial elections offer better conditions for directly testing turnout effects in U.S. politics. In a comprehensive analysis of these statewide elections since 1928, we find that the conventional theory was true outside the South through 1964, but since 1965 the overall relationship between turnout and partisan outcomes has been insignificant. Even before the mid-1960s, the turnout effect outside the South was strongest in Republican states and insignificant or negative in heavily Democratic states. A similar but weaker pattern obtains after 1964. In the South, which we analyze only since 1966, higher turnout helped Republicans until 1990, but in 1990–94 the effect became pro-Democratic. The conventional theory cannot account for these complex patterns, but they are impressively consistent with DeNardo's (1980) theory.

With a jump in turnout in 1992 and implementation of the National Voter Registration Act (NVRA) in 1995, the United States may have entered a new era of higher voter participation. If so, a period of three decades during which steadily declining turnout fascinated and alarmed political analysts will come to a close.[1] Although many observers value a high rate of voting as a systemic requirement of a healthy democracy, much of the debate over low U.S. turnout has focused on, or been motivated by, its putative partisan consequences. Echoing a familiar staple of election-day commentary—the notion that low turnout helps Republican candidates—influential writers have contended that a shrunken electorate has been an important factor in Republican electoral advances and the shift of U.S. politics to the right (e.g., Burnham 1987, Edsall 1984, Piven and Cloward 1988). Politicians apparently accept the conventional theory as well. Early versions of the NVRA (popularly known as the "motor voter" bill) were vetoed by Republican President George Bush or stymied by Republican filibusters in the Senate (Wolfinger 1993). A Democratic Congress and president finally enacted the law in 1993 with the aid of only a few Republicans. Subsequently, many Republican governors refused to implement the new law until forced to do so by the courts (Earle 1995, Piven and Cloward 1996).

Considering the practical importance of the issue and the simplicity of the underlying proposition (higher turnout helps Democrats), direct tests of the conventional theory are surprisingly scarce. Most scholars who challenge the theory have relied on indirect inferences from surveys that show voters and nonvoters differing little, if at all, in partisan identification and political attitudes (e.g., Texeira 1992, 137–43, Wolfinger and Rosenstone 1980, 109–14). We know of only two tests based on votes cast in real elections rather than attitudes expressed in surveys.[2] These studies reach opposite conclusions; and, for different reasons, neither can be regarded as definitive. DeNardo (1980), in an analysis designed to test his mathematical model of turnout effects (about which more below), investigates more than 300 House elections between 1938 and 1966. Regressing the Democratic share of the vote against voter turnout,[3] he finds that the conventional hypothesis of a positive relationship is supported for districts in which Democrats were a minority of registered voters, but in districts dominated by Democrats, the relationship became insignificant or negative, depending on the size of their majority. This pattern held for elections in 1938, 1946, 1950, and 1954, but it became inconsistent in 1962; in 1966, no significant relation obtained at any level of Democratic registration. Radcliff (1994), who studied state-level results for presidential elections from 1928 through 1980, found a weak or insignificant relationship between turnout and the Democratic vote before 1960 but a strongly positive relationship in 1960–80—the reverse of the pattern in DeNardo's House data.[4]

Elections for the House and presidency present draw-

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[1] Measured as a percentage of the voting-age population (VAP), turnout in presidential elections rose to 55.2% in 1992 after falling from 63.8% to 50.1% between 1960 and 1988 (Abramson, Aldrich, and Rohde 1995, 102). Predictions of turnout under the NVRA vary widely. Projecting cautiously from pre-NVRA experience in the states, Rhine (1995) concludes that the NVRA has the potential to increase turnout only "by several percentage points." Extrapolating from early experience with the new law, Piven and Cloward (1996) project an increase of 40 million in registration by 1998. If 70% of new registrants vote, then that estimate implies a turnout in the 2000 presidential election of about 65% of VAP.

[2] A similar situation obtains in comparative politics. After surveying the literature, Crewe (1981, 253) wrote: "There is an honored place in election folklore for the view that the share of the vote going to workers' parties fluctuates in tandem with turnout. I know of no academic study which has demonstrated this to be true for any country." Subsequently, two studies produced evidence supporting the conventional theory: Nagel (1988) for New Zealand and Pacek and Radcliff (1995) for 19 industrial democracies. Both papers argue that the turnout-vote relationship varies, depending on the extent to which partisan preferences are linked to socioeconomic status.

[3] Actually, the negative inverse of turnout. See footnote 6.

[4] Erikson (1995a, 1995b) has criticized Radcliff's work on methodolog-

backs for tests of turnout hypotheses. Many House districts rarely see serious competition between the parties. Because of their socioeconomic composition, safe Democratic and Republican districts tend to have low and high turnout, respectively, thus making cross-sectional tests problematic. Furthermore, extended time-series tests are impossible for most congressional districts because of decennial boundary changes. Presidential elections also furnish an imperfect site for testing turnout hypotheses. If aggregate national results are used, the number of cases is small. Radcliff's innovation of using states as the unit of analysis creates numerous data points, but the observations in any given year are not independent, because turnout and the vote in all fifty states are strongly affected by the same national influences—the economy, political events, issues, and candidates.

Besides being important in their own right, senatorial and gubernatorial contests provide better conditions for testing turnout hypotheses in the U.S. context. The units (states) are fixed and politically meaningful. These statewide elections combined are approximately twice as frequent per state as presidential races, producing a large number of observations. Although national events affect senatorial and gubernatorial contests, their outcomes in any given year are more independent of these than are state-level presidential results. Since the development of a two-party South, nearly all states have experienced serious partisan competition in statewide contests (over a reasonable span of time, if not in every race)—more so than in presidential contests and far more so than in most House districts. Overall, senatorial and gubernatorial contests offer a desirable combination of high visibility, significant local variation, and strong partisan cues.

This paper presents an analysis of turnout effects in U.S. senatorial and gubernatorial elections from 1928 through 1994. Our investigation began as a straightforward test of the conventional theory. Specifically, we asked whether Democratic candidates generally have won a larger share of the vote when turnout has been higher. We found that to be the case from 1928 through 1964, but turnout fluctuations after 1965 had no significant effect on party fortunes except in the South, where higher turnout helped Republicans until 1990 and Democrats thereafter. Moreover, even in the 1928–64 period, a strongly significant pro-Democratic turnout effect appeared in only nine states, most of which were predominantly Republican. The conventional turnout hypothesis cannot explain these complex patterns. Therefore, we turned to its chief rival, a mathematical model developed by DeNardo (1980), which we dub the two-effects theory. In sometimes unexpected and nonobvious ways, our data confirm the highly contingent predictions of the two-effects model.

The paper is organized as follows. The first section describes our data and methods. We then present results from five analyses: an overall pooled time series, separate year-by-year regressions, a pooled time series based on division of the data into early and recent periods, state-by-state regressions, and a separate study of the South since the Voting Rights Act of 1965. In the third section, we explain our results by answering four perplexing questions about our basic findings. To do so, we reconsider the conventional and DeNardo models. After explicating each in detail, we develop their implications for our data and test their ability to explain our four puzzles. In every case, our results uphold the DeNardo model. Though not originally designed for this purpose, the present study thus provides compelling confirmation of DeNardo's theory. In the final section, we consider theoretical, practical, and ideological implications of this conclusion.

DATA AND METHODS

Our study includes all senatorial and gubernatorial elections since the beginning of the New Deal realignment in 1928, with two exceptions. (1) Prior to implementation of the 1965 Voting Rights Act, discriminatory barriers prevented many southern blacks from voting, and most southern states lacked a competitive two-party system. As a result, those states exhibited a pattern of low turnout and high Democratic vote shares, which would militate against the conventional turnout hypothesis for reasons unique to that region. Therefore, we eliminated all elections in the eleven states of the Confederacy from 1928 through 1965. (2) To avoid the complications of multicandidate races, we restricted the analysis to straight two-party contests by dropping about 300 elections in which a third-party candidate received more than 5% of the vote or in which votes for third-party candidates exceeded the winner's margin of victory. The remaining database comprises 1,842 cases—983 for senator and 859 for governor.[5]

Our main dependent variable is the Democratic candidate's percentage of the total vote (the *Democratic Vote Share*). Because we excluded races with significant third-party candidates, the Republican vote share is always close to 100 minus the Democratic vote share. Our principal independent variable is *turnout*, defined as the sum of Democratic and Republican votes divided by the voting-age population (VAP) of the state.[6] Basing turnout on VAP rather than registered voters is standard practice in comparative studies (e.g., Jackman 1987, Powell 1986) and is virtually a necessity for the United States because of varying registration laws and administrative practices. After experimenting with several controls, we found that incumbency effects were nearly always significant, so most of our results include dummy

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ical grounds. We develop our own refutation of Radcliff in a separate paper (Nagel and McNulty 1996).

[5] Elections in which a state filled both offices in one year are treated as two observations.

[6] DeNardo's model predicts that the Democratic vote share will be a linear function of the negative inverse of turnout (−1/turnout) rather than of turnout directly. Therefore, we estimated our principal equations using both dependent variables. The negative inverse offers a slightly better fit (as measured by the *t* ratio) for most of our main regressions in tables 1 and 2. For ease of interpretation and because the differences are so minor, we report results for turnout only. In doing so, we do not question DeNardo's mathematics, but we suspect that most of the nonlinearity he predicts occurs outside the observed range of our data.

variables for the presence or absence among the candidates of a Democratic or Republican incumbent.[7]

Taken as a whole, our database constitutes a pooled time series (Sayrs 1989, Stimson 1985). As such, it offers the advantage of a very large number of observations but also poses the risk that ordinary-least-squares estimates from the pooled data may be biased by between-unit (states) or between-time-point (years) effects. Unit differences are an especially troubling problem in research on turnout, because if (as is widely believed) Republicans are more likely to vote than Democrats, then states with more Republicans will usually have higher rates of turnout (Grofman 1993, 98). Pooled regressions that fail to control for partisan differences across states thus will be biased against the conventional hypothesis. Consequently, our basic pooled estimates controlled for state effects by using least-squares with dummy variables specifications that included dummies for every state except one. We then searched for time effects by estimating separate cross-sectional models for all even-numbered years. The resulting time series of coefficients revealed a shift in the relationship between turnout and party fortunes after 1964. Therefore, we estimated new pooled models for 1928–64 and 1965–94. We then turned to a state-by-state analysis, estimating separate equations for each state before and after 1964. Finally, we undertook a separate analysis for the eleven southern states for the period after the Voting Rights Act took effect.

RESULTS

In this section, we report the results of each stage of our analysis, postponing all efforts at interpretation and explanation until the full pattern of empirical evidence has been presented.

Pooled Estimates, 1928–94

Our estimates for the pooled time series are based on the following specification:

$$\textit{Democratic vote share} = a + b_1\textit{Turnout} + b_2D + b_3R + \Sigma c_i\, S_i, \qquad (1)$$

where D and R are dummy variables indicating the candidacy of Democratic and Republican incumbents, respectively, and the S_i are dummy variables for states (omitting Wyoming, the victim of alphabetical tyranny). Alaska and Hawaii are included beginning in 1958 and 1959, respectively, when they held their first elections as states. In the results reported here, we omit the eleven southern states for two reasons: (1) to preserve consistency with later temporal comparisons, in which the South must be omitted because it does not appear in our data set until 1966, and (2) because, even after 1966, the South exhibits a pattern different from the rest of the United States. Accordingly, we analyze the South in a separate section below. Thus, Table 1 reports results outside the South for senatorial and gubernatorial elections combined and for each office separately.[8]

In all three regressions, the effect of turnout on the Democratic share of the vote is positive and significant, although it is stronger for senators and for the two offices combined than for governors. Thus, if one were to draw conclusions from this initial pooling of data from nonsouthern states and the entire 1928–94 time span, it would appear that the evidence supports the traditional model. On the whole, Democratic candidates for senator and governor fared better when turnout was higher. Although not huge, the effect is statistically significant, and it could change the outcome of a close race. (A 10% rise in turnout would produce on average a 0.75% increase in the Democratic vote share, which implies a 1.5% gain for the Democrat relative to the Republican in a two-candidate race.) It would be a mistake, however, to apply this conclusion to contemporary politics without first checking for possible changes in the turnout-vote relationship over time.

Year-by-Year Results

In a design of least-squares with dummy variables, it is possible to control simultaneously for unit and time effects. Radcliff (1994) does so in estimating a pooled model for the vote-turnout relation in presidential elections. Stimson (1985, 922) advises against this practice because "it may curtail our ability to model either [time or space] dimension of the phenomenon in question by assigning much of its systematic variation to dummy atheoretical variables which are collinear with explanatory variables." Colinearity between time and turnout exists in our data set because, as is well known, turnout declined in the United States from 1960 through 1988. Moreover, incorporating years as dummies detects only additive (intercept) effects of time on vote shares, whereas we are more interested in interaction (slope) effects.[9] Specifically, during the 66-year span of our data, was the relationship between turnout and the vote reasonably constant, or did it change significantly?

In a preliminary, exploratory attempt to answer this question, we estimated separate regression equations for every even-numbered year from 1928 through 1994. (Odd-numbered years average little more than one election each in our database.) To keep the units comparable over time, we excluded the South through-

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[7] We obtained most of our election data from *Congressional Quarterly's Guide to U.S. Elections*, 2nd ed. (Washington: Congressional Quarterly, 1985). Other results came from Michael Barone and Grant Ujifusa, *The Almanac of American Politics 1992* (Washington: National Journal, 1991); *Information Please Almanac, Atlas, and Yearbook*, 45th and 46th eds. (Boston: Houghton Mifflin, 1992, 1993); *Congressional Quarterly Weekly Reports* (11/7/92 and 11/12/94); the *New York Times* (various dates); and a computer software package, *The Great American History Machine* (College Park, MD: Academic Software Development Corp., 1991). We took VAP figures for each state from U.S. Bureau of the Census, *Current Population Reports*, series P-25 (1944–91). For non-Census years, we estimated the population by interpolating or extrapolating using a standard logarithmic formula (Barclay 1958, 31-3).

[8] Including the post-1965 southern observations slightly reduces the turnout coefficients reported in Table 1 but makes no qualitative difference in the findings.

[9] Radcliff (1995) now agrees that year dummies should not be used.

TABLE 1. Effect of Turnout on the Democratic Vote in Nonsouthern States, 1928–94

Offices	Turnout	Democratic Incumbent	Republican Incumbent	Adjusted R^2	Number of Cases
Senators and governors	7.50***	9.47***	−7.52***	.51	1672
	(1.62)	(0.42)	(0.43)		
Senators only	9.33***	10.02***	−7.68***	.56	895
	(2.25)	(0.59)	(0.61)		
Governors only	6.30**	8.69***	−6.86***	.45	777
	(2.40)	(0.64)	(0.63)		

Note: Estimates are by least-squares with dummy variables regression. The dependent variable is the *Democratic Percentage of the Vote*, with a range of 0 to 100. *Turnout* is measured as a ratio and ranges from 0 to 1. Cell entries are unstandardized regression coefficients with standard errors in parentheses. The table omits intercepts and coefficients for the 38 state dummies.

$^{**}p < .01$ (two-tailed)

$^{***}p < .001$ (two-tailed)

out.[10] The number of elections per year ranged from 31 (in 1992) to 61 (in 1962), with a mean of 48. For each year, we regressed the Democratic vote share on turnout and the two incumbency dummies. The incumbency effects were nearly always significant, but our focus is on the effect of turnout over time.

As Figure 1 shows, regression coefficients between turnout and the Democratic vote fluctuated widely from year to year. Because the number of cases each year is relatively small and unit effects cannot be controlled, not much confidence can be placed in the annual estimates per se. Nevertheless, their overall pattern suggests that a fundamental shift occurred. Of the five significant ($p <$ .05, two-tailed) positive coefficients, four appeared before 1964 and only one afterward—the anomalous spike in 1972. Looking just at the signs of coefficients, without regard to statistical significance, through 1964, 12 of 19 coefficients (63%) are positive; after 1964, 12 of 15 (80%) are negative.[11] Consequently, in the next section we estimate new pooled regressions for the early and recent periods, using 1964 as the cutpoint.[12]

Pooled Results for 1928–64 and 1965–94

Table 2 reveals that our previous finding of a positive overall turnout effect for the entire period since 1928 is seriously misleading. For 1928 through 1964, the relationship between turnout and the Democratic vote was highly significant and more than twice as strong as the overall result reported in Table 1. Moreover, the coefficients for senatorial and gubernatorial elections were virtually identical in the early period, whereas for 1928–94 the former were stronger than the latter. The turnout effect through 1964 was strong enough to matter in practical politics—a 10% increase in turnout was associated on average with about a 3.5% change in the party differential in favor of the Democrats.

The data since 1964 tell a different story. For the Senate, there was no relation at all between turnout and the Democratic vote. In gubernatorial elections, Republicans rather than Democrats actually fared better when turnout was higher, though the negative coefficient fell short of statistical significance ($p = .15$). For the two offices combined, the coefficient was again negative but insignificant, with $t < 1$. In short, for senatorial and gubernatorial elections in 37 nonsouthern states (excluding Alaska and Hawaii), there *was* a strong positive relation between turnout and the Democratic share of the vote from 1928 through 1964. After 1964, the turnout effect vanished.[13]

Controlling for Presidential Elections

All our regressions thus far pooled elections for presidential and nonpresidential years. Turnout is typically lower in nonpresidential years, and the class composition of electorates in the two sets of years probably differs as well.[14] Therefore, it might be supposed that the partisan effects of turnout differ depending on whether a presidential race tops the ballot. Moreover, cues provided by presidential contests may spill over to influence both turnout and partisan outcomes in senatorial and gubernatorial elections, thus possibly contaminating the estimates reported above.

To check for these possibilities, we separated our data set into presidential and nonpresidential election years and then carried out estimates for the full period and the

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[10] The regressions reported in Table 1 do, however, include Alaska and Hawaii from 1958 onward.

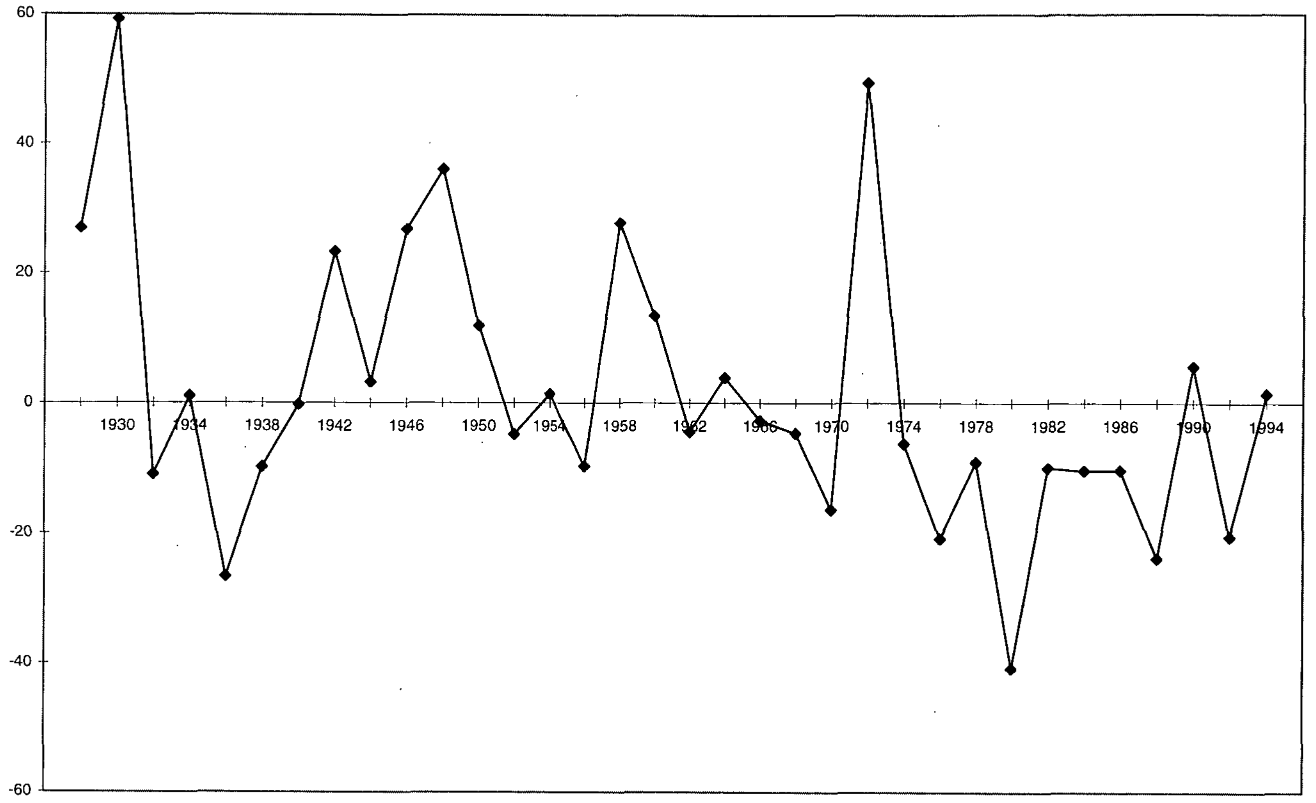
[11] None of the negative coefficients reached the .05 level of significance, although 1936 and 1980 were just short of it. Four of the seven negative coefficients before 1965 occurred in 1932–40 (with a minuscule positive coefficient in 1934). When elections from this decade were pooled in a separate regression, however, the turnout effect was positive, though statistically insignificant ($t = 1.38, p = .17$) and much weaker than for the rest of the 1928–64 period. For an explanation of the 1930s anomaly, see footnote 23.

[12] Using the sign test, 1960 offers an almost equally good cutpoint—65% of coefficients through 1960 are positive, and 76% after that year are negative; but pooled regressions yield slightly stronger results when the data are divided after 1964.

[13] This dramatic change is all the more puzzling because Radcliff (1994) reports the *opposite* pattern for state-level turnout effects in presidential elections—an insignificant relation from 1928 through 1956, and a strong positive association with the Democratic vote from 1960 through 1980. Nagel and McNulty (1996) show that Radcliff's result depends on his arbitrary division of the data set after 1956. Using the same specification and methods as in this paper, we found the same pattern of turnout effects for presidential elections as for senatorial and gubernatorial races.

[14] Rosenstone and Hansen (1993, 273, 282) show that education and employment have a stronger effect on turnout in midterm than in presidential elections, but the reverse is true for income.

FIGURE 1. Year-by-Year Regression Coefficients Relating Turnout to Democratic Vote, 1928–94



two subperiods. These results are displayed in Table 3. The pattern of turnout effects remains unchanged compared with our previous estimates. For the full period, 1928–94, turnout had a statistically significant positive effect on the Democratic vote share in both presidential and nonpresidential years, but these relatively weak coefficients again mask a change that occurred about 1964. For elections from 1928 through 1964, the turnout effects were strongly positive in both presidential and nonpresidential years. After 1965, the effect of turnout

TABLE 2. Effect of Turnout on the Democratic Vote during Early and Recent Periods in 37 Nonsouthern States

Period and Offices	Turnout	Democratic Incumbent	Republican Incumbent	Adjusted R^2	Number of Cases
1928–64					
Senators and governors	16.42*** (2.08)	7.28*** (0.52)	5.73*** (0.50)	.54	989
Senators only	17.39*** (2.99)	6.49*** (0.75)	−6.00*** (0.75)	.56	506
Governors only	17.62*** (3.06)	7.75*** (0.77)	−5.32*** (0.71)	.50	483
1965–94					
Senators and governors	−3.03 (3.29)	10.24*** (0.68)	−9.27*** (0.71)	.56	647
Senators only	−0.08 (4.19)	11.25*** (0.92)	−7.71*** (0.95)	.62	365
Governors only	−8.80 (6.06)	8.58*** (1.13)	−10.01*** (1.22)	.47	282

Note: Estimates are by least-squares with dummy variables regression. Alaska, Hawaii, and the eleven southern states are omitted. The dependent variable is the *Democratic Percentage of the Vote*, with a range of 0 to 100. *Turnout* is measured as a ratio and ranges from 0 to 1. Cell entries are unstandardized regression coefficients with standard errors in parentheses. The table omits intercepts and coefficients for the 36 state dummies.
***$p < .001$ (two-tailed)

TABLE 3. Effect of Turnout on the Democratic Vote for Presidential and Nonpresidential Election Years in Nonsouthern States

Period	Turnout	Democratic Incumbent	Republican Incumbent	Adjusted R^2	Number of Cases
1928–94					
Presidential years	11.36*** (3.20)	9.52*** (0.63)	−8.04*** (0.63)	.53	754
Nonpresidential years	13.16*** (2.77)	9.69*** (0.59)	−6.91*** (0.60)	.50	918
1928–64					
Presidential years	20.98*** (4.60)	7.56*** (0.71)	−6.14*** (0.71)	.54	498
Nonpresidential years	34.42*** (3.73)	7.20*** (0.75)	−5.24*** (0.72)	.58	491
1965–94					
Presidential years	−0.19 (8.60)	11.66*** (1.17)	−10.04*** (1.22)	.56	249
Nonpresidential years	−5.44 (5.43)	9.21*** (0.89)	−8.89*** (0.93)	.54	398

Note: Estimates are by least-squares with dummy variables regression. The eleven southern states are omitted throughout. Alaska and Hawaii are included in the full (1928–94) analysis but are omitted from the subperiod analyses. The dependent variable is the *Democratic Percentage of the Vote*, with a range from 0 to 100. *Turnout* is measured as a ratio and ranges from 0 to 1. Cell entries are unstandardized regression coefficients with standard errors in parentheses. The table omits intercepts and coefficients for the state dummies.
***$p < .001$ (two-tailed)

on the Democratic vote was statistically insignificant in both presidential and nonpresidential years.

Within this overall pattern of qualitative consistency with our previous findings, there is some quantitative evidence in favor of the hypothesis that influences from presidential elections can interfere with the turnout-vote relation in statewide contests. During the period when the turnout effect prevailed (1928–64), it was nearly twice as strong in nonpresidential years. In these contests, an increase of 10% in turnout produced an average swing of almost 7% in the two-party differential toward the Democrats—a powerful effect that could change the outcome in a great many elections. It is also noteworthy that controlling in this way for presidential elections increases the magnitude of the turnout coefficients in both presidential and nonpresidential years compared with the combined result reported in the first row of Table 2.

State-by-State Results

Is the difference between early and recent periods replicated in individual states? To answer this question, we regressed the Democratic vote share on turnout and (where possible) incumbency dummies for each of the 37 states in the two periods. The turnout coefficients are reported in Table 4, in which the states are ordered according to the value of their turnout effects in each period. With fewer cases per run (17–35 in the early period, 10–23 in the recent), only 11 of 74 turnout coefficients were statistically significant, but the pattern of state results is consistent with the pooled regressions. As the summary in Table 5 shows, coefficients for 1928–64 were overwhelmingly positive and more often significant. For 1965–94, coefficients were almost all insignificant, with a majority of negative signs.

Nevertheless, the relationships between turnout and voting outcomes were far from uniform across the states. Even in the early period, when pooled results were strong, most states showed a statistically insignificant effect. As can be seen from Table 4, one could confidently assert that the conventional hypothesis was true for only nine seemingly disparate states—Oregon, California, Pennsylvania, Colorado, Vermont, Michigan, Maine, Iowa, and South Dakota. In some cases, including Illinois and New York, the relationship was negative. Moreover, state turnout effects did not decline uniformly between the two periods. Early and recent rankings of states by regression coefficients (displayed in Table 4) appear unrelated, an impression that is verified by the 0.085 correlation between states' coefficients for the two periods. Is there any way to bring theoretical order to these seemingly random patterns? There is, but before presenting it, we turn to the heretofore neglected South.

Results for the South

Thus far, we have excluded the 170 southern elections in our database from all the pooled regressions. One reason is that our southern data do not begin until 1966, but there is another justification: Despite the decline in its political particularity since 1965, the South remains a distinctive region. Over the past three decades (1966–94), it continued to exhibit both lower turnout and greater Democratic success in senatorial and gubernatorial elections relative to the rest of the United States. Turnout in these statewide contests averaged 38% in the South compared with 50% outside the South; the mean

TABLE 4. Turnout Effects and Average Democratic Vote for Early and Recent Periods in Nonsouthern States

1928–64				1965–94			
State	Turnout Coefficient (Standard Error)		Average Democratic Vote	State	Turnout Coefficient (Standard Error)		Average Democratic Vote
OR	69.45***	(13.93)	42.10	AK	213.44	(265.07)	44.20
CA	66.37***	(12.35)	43.04	ME	61.25	(70.02)	52.00
PA	40.47*	(15.22)	46.12	NY	60.03x	(34.11)	53.99
CO	31.55***	(8.59)	51.70	UT	39.74x	(18.60)	45.09
VT	31.22**	(9.13)	36.63	WI	23.26x	(13.69)	53.12
MI	30.55**	(11.02)	46.39	SD	20.09	(33.71)	45.84
ME	29.57*	(12.98)	43.40	VT	16.00	(21.55)	47.44
IA	26.46*	(7.55)	47.73	MO	14.07	(20.97)	47.44
SD	24.53*	(12.79)	44.56	NH	14.05x	(13.41)	44.66
ND	22.26x	(18.63)	41.42	NJ	12.95	(22.26)	51.03
ID	20.82x	(14.22)	47.39	CA	11.46	(14.16)	48.73
CT	17.87x	(12.21)	49.71	MN	9.31	(30.66)	48.66
KS	17.05x	(8.49)	43.94	NM	8.73	(25.46)	48.41
NJ	15.70x	(9.52)	48.07	IL	2.50	(13.00)	49.42
UT	15.46x	(10.69)	52.33	WA	−0.99	(17.89)	54.38
MD	12.71	(24.29)	54.56	IN	−1.47	(13.21)	45.04
WY	12.04x	(11.62)	50.11	ID	−2.00	(22.29)	49.25
DE	9.71	(11.72)	47.50	AZ	−3.01	(19.74)	48.32
MT	9.37	(36.90)	54.45	CO	−3.41	(24.00)	50.32
WA	8.85	(16.92)	55.04	OR	−4.74	(11.98)	45.07
WV	7.23	(10.09)	54.27	MI	−7.59	(18.14)	50.48
KY	6.52	(7.66)	53.45	DE	−7.77	(28.00)	46.60
IN	5.68	(14.41)	46.68	KS	−7.99	(32.96)	42.23
AZ	4.69	(27.97)	58.82	MA	−8.40	(21.63)	52.53
OK	3.99	(16.09)	55.63	WY	−11.28	(21.19)	44.01
MN	3.25	(19.47)	49.08	NV	−11.66	(23.46)	52.52
NE	2.92	(12.62)	42.66	OH	−13.38	(30.89)	51.41
MA	2.04	(14.51)	51.30	WV	−15.99	(25.35)	59.79
NH	1.64	(9.20)	45.32	IA	−17.77x	(12.35)	45.27
WI	1.61	(14.74)	46.30	PA	−27.75x	(21.92)	48.09
OH	0.89	(10.86)	49.20	CT	−30.48*	(12.33)	55.67
NM	−0.93	(9.25)	53.53	MT	−35.39x	(31.05)	55.44
MO	−1.75	(12.18)	55.52	MD	−39.10	(46.20)	58.67
NV	−6.28	(18.89)	56.07	KY	−39.47x	(19.21)	56.19
NY	−6.38	(9.87)	47.33	ND	−40.10*	(17.63)	52.63
RI	−9.85	(17.73)	55.74	OK	−45.88x	(34.33)	53.56
IL	−17.32x	(12.02)	50.85	RI	−47.15x	(30.04)	56.51
				HI	−70.70x	(64.35)	63.35
				NE	−71.55x	(47.90)	52.97

x $|t| > 1$; *$p < .05$; **$p < .01$; ***$p < .001$ (two-tailed)

Democratic vote shares were 54.6% in the South and 50.4% elsewhere. Democrats won 61.8% of southern and 51.8% of nonsouthern senatorial and gubernatorial races. Inclusion of the South with the other states, therefore, will bias turnout effects downward, militating against the conventional hypothesis for reasons that may tell us more about the region's unique historical legacy than about the validity of the hypothesis.

Rather than ignore the South, we propose to treat it separately.[15] Our investigation follows the same sequence as our analysis of the non-South. First, we estimated a pooled least-squares model with dummy variables for the entire data set. Next, we computed year-by-year results and searched for cutpoints at which the relationship changed. Finally, we estimated separate regressions for each state to see if the pooled pattern prevailed in the separate units. The first row of Table 6 gives results for the pooled model over the entire 1966–94 period. Turnout was negatively and significantly related to the Democratic vote—Democrats did *worse* in the South when turnout was high. As a comparison, we also estimated a pooled model for the nonsouthern states (including Alaska and Hawaii) over the same time span. The regression coefficient outside the South also was negative but much smaller (−3.47), and despite four times as many cases it fell well short of statistical significance ($t = -1.04$, $p = .3$).

Is the negative southern turnout effect stable across units and over time? As Table 7 shows, state-by-regres-

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[15] Whether to use data from the South is a point of dispute in the controversies between Tucker and Vedlitz (1986) and DeNardo (1986) and between Erikson (1995) and Radcliff (1995).

TABLE 5. State Turnout Regression Coefficients in Two Periods

	1928–64	1965–94
All coefficients		
Positive	31	15
Negative	6	22
Coefficients with $\|t\| > 1$		
Positive	16	4
Negative	1	9
Significant coefficients*		
Positive	9	0
Negative	0	2

Note: Cell entries are numbers of state turnout coefficients.
*$p < .05$ (two-tailed)

sions yielded negative coefficients for all southern states except three (Louisiana, Texas, and Virginia). With small *N*s (from six to twenty one), the turnout effect reached the two-tailed 0.05 level of significance in only one state (Alabama); but most of the coefficients were numerically large, and *t* ratios exceed one in the majority of cases.

Year-by-year estimates for the South were also unreliable due to small numbers (*N*s for even years ranged from 5 to 15, with a mean of 10). Nine of the annual coefficients were negative, and six were positive. Only one estimate was significant at the 0.05 level—a strongly positive coefficient for 1972, corresponding to the spike in Figure 1 for the non-South in that year. Unlike the pattern outside the South since 1966, however, the positive southern coefficients were more frequent and clustered in two groups—1972–74–76 and 1990–92–94. In a pooled analysis of the first cluster, the turnout coefficient was insignificant but negative; but a separate pooling for 1990–94 resulted in the significant positive turnout effect reported in the third row of Table 6.[16] Removing 1990–94 from the larger data set also substantially strengthened the negative turnout effect for the remaining years, as can be seen by comparing the first and second row of Table 6. We conclude, therefore, that high turnout in the South favored Republican senatorial and gubernatorial candidates from 1966 through 1989, but from 1990 through 1994, Democrats gained from higher turnout. For the rest of the United States, in contrast, a comparable shift did not occur. The pattern of signs was the same—negative before 1990 and positive afterward—but in neither period did the coefficients for the non-South even approach statistical significance.

EXPLAINING THE RESULTS

The preceding tour of the evidence leaves us with four puzzling questions. (1) Why was the turnout-vote relation outside the South strongly positive through 1964 but insignificant or negative thereafter? (2) During most of the last three decades, why was the turnout effect significantly negative in the South but not in the rest of the United States? (3) Why did the southern coefficient shift signs in 1990–94, while the turnout effect in the non-South remained insignificant? (4) Can we explain the great variation in state turnout coefficients, including their changes across periods?[17]

Ideally, a theory of the turnout-vote relation should be able to answer all these questions. We wish to assess the relative performance of two main contenders: the conventional theory and DeNardo's model. In such a comparison, the conventional theory is at a disadvantage, because it is usually stated as a simple linear hypothesis (or article of faith), rather than as a fully developed theory. In fact, so far as we know, the most rigorous specification of the conventional argument comes from DeNardo himself, who incorporates it as a limiting case within his more general model. To give the conventional theory its best possible advantage, we shall elaborate it by melding DeNardo's formalization with additional premises.

The Conventional Theory

A loosely stated version of the conventional theory might run as follows. (1) Voting participation is associated with socioeconomic status (SES), such that people with higher SES vote more reliably than do those with lower SES. (2) Therefore, when turnout rises, it is probably because there are more low-SES voters than usual. (3) These sporadic low-SES voters are more likely to vote for the party whose economic policies are more in their interest, which in the United States is the Democratic Party. (4) Therefore, higher turnout helps Democrats.

DeNardo's formalization of the conventional theory divides the electorate into two base camps, one normally inclined to the Democrats (B_d), the other to the Republicans (B_r). Each base can be further divided into the core (C_d, C_r) and the periphery (P_d, P_r). The core participates reliably, and the periphery does not. DeNardo shows that if rates of defection (members of one party's base voting for the other party) stay below 50%, then the turnout effect (given by the partial derivative of vote shares with respect to changes in turnout) is a function of the peripherality ratio (P_d/P_r). The greater this ratio, the stronger the turnout effect, but Democrats will reap some benefit from higher turnout as long as a larger fraction of the Democratic base consists of peripheral voters ($P_d/P_r > 1$). DeNardo's model also implies that the turnout effect will weaken (1) as the general level of turnout rises, (2) as defections in either or both camps increase, and (3) as the partisan division of eligible voters becomes extremely skewed (DeNardo 1980, 411).[18]

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[16] The number of cases for 1990–94 (35) was too small to yield reliable results using the least-squares method with dummy variables. (Except for Republican incumbency, every variable, including the nine dummies, had an insignificant coefficient.) Results in Table 6 for this period are from an ordinary-least-squares estimate, without state dummies.

[17] Nagel and McNulty (1996) deal with a fifth puzzle: Why is the pattern Radcliff (1994) reports for presidential elections the exact opposite of the one we found for senatorial and gubernatorial contests? (See footnote 13.)

[18] DeNardo's model abstracts from any mention of social class or other

TABLE 6. Effect of Turnout on the Democratic Vote in the South

Period	Turnout	Democratic Incumbent	Republican Incumbent	Adjusted R^2	Number of Cases
1966–94	−23.18* (9.17)	7.85*** (1.46)	−11.55*** (1.94)	.48	170
1966–89	−37.74*** (9.49)	9.72*** (1.52)	−10.89*** (2.12)	.55	135
1990–94	36.09* (14.68)	6.05 (3.03)	−9.40* (3.60)	.43	35

Note: 1966–94 and 1966–89 were estimated using least-squares dummy-variables regression; 1990–94 was estimated using ordinary least-squares. The dependent variable is the *Democratic Percentage of the Vote*, with a maximum range of 0 to 100. *Turnout* is measured as a ratio and ranges from 0 to 1. Cell entries are unstandardized regression coefficients with standard errors in parentheses. Intercepts and coefficients for state dummies are omitted.
*$p < .05$ (two-tailed)
***$p < .001$ (two-tailed)

DeNardo's Two-Effects Theory

Critics refer to DeNardo's theory as the "defection model" (Radcliff 1994, Tucker and Vedlitz 1986), but we prefer to call it the two-effects model because the full theory incorporates both conventional and defection hypotheses. The two-effects theory differs from DeNardo's formalization of the conventional model only by adding the assumption that peripheral voters are more likely than core voters to defect to the other party. If so, then two countervailing effects occur as turnout increases. Through the *composition* effect, increased participation among peripheral voters helps the party with the larger proportion of peripheral voters—according to the conventional assumption, the Democrats ($P_d > P_r$). Yet, higher defection rates among those peripheral voters hurt the party with the larger absolute number of peripheral voters, which depends on the products P_dB_d and P_rB_r. Consequently, this *defection* effect tends to hurt the party with the larger base, which varies from district to district and over time. In heavily Republican political units, the two effects work together, so higher turnout strongly favors Democrats. In heavily Democratic electorates, however, the two effects work against each other, so the absolute magnitude of the turnout effect is smaller in Democratic than in Republican districts. As the Democratic majority grows, the defection effect eventually surpasses the composition effect, changing the sign of the overall turnout-vote relation, so that higher turnout actually helps Republicans. Where the parties' bases are roughly equal, the composition effect should prevail, so higher turnout will help Democrats but less strongly than in places with large Republican majorities. Finally, DeNardo (1980, 417) shows that as the proportion of core voters shrinks toward zero (i.e., nearly everyone becomes an unreliable, sporadic voter), the turnout effect (whether positive or negative) withers away, because the composition and defection mechanisms both "hinge on differences in the behavior of core and peripheral voters."

Figure 2 summarizes the predicted relations between the turnout effect and B_d, the size of the Democratic base (i.e., the proportion of the electorate normally inclined to favor Democrats). According to the conventional model as elaborated by DeNardo (Figure 2a), the turnout effect is positive at all values of B_d, though it approaches zero when one party or the other has an overwhelming majority (B_d is near 0 or 1). Except near these extreme values of B_d, the turnout effect should be approximately constant.[19] In contrast, according to the two-effects model (Figure 2b), through most of the range of B_d, the turnout effect declines monotonically as B_d increases, and it turns negative at $B_d{}^*$, where $B_d{}^* > 0.50$.

Testing between the two models thus requires an indicator of B_d, the size of the Democratic base. In his original test, DeNardo (1980) used the proportion of registered voters enrolled as Democrats in a sample of House districts. Partisan enrollment data are often unavailable or unreliable, and we made no attempt to gather such figures for the 50 states and 67 years represented in our data. In his study of presidential elections, Radcliff (1994) uses two operationalizations of B_d for each state: the Democratic share of the vote in the midterm election two years before each presidential contest, and a moving average of immediately prior midterm and presidential votes. Because our data include midterm elections, Radcliff's first method is not available to us. Although we might have constructed some variant of a moving-average indicator, we decided to use a simpler approach. Because the comparisons of interest involve extended periods, we assume that the Democratic and Republican bases during any interval are indicated by the mean Democratic and Republican vote percentages over that period in the units we are analyzing. Relying on this serviceable though imperfect assumption,[20] we now proceed to apply the conventional

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causes of differences in the peripherality ratio, but his argument might be extended to incorporate such factors. Specifically, we would expect P_d/P_r and, therefore, the turnout effect to increase with the degree of class voting (Pacek and Radcliff 1995), which in turn should be a function of the salience of issues affecting economic security and equality and the perceived difference between the parties on such issues. Conversely, P_d/P_r will decline to the extent that the Democratic party succeeds in consistently mobilizing lower-SES voters at high rates, because then they become part of the Democratic core, and P_d falls.

[19] DeNardo (1980) also predicts that the turnout effect will decline as the general rate of turnout increases. This relationship is not depicted in Figure 2, but we refer to it in footnote 23.

[20] DeNardo (1986) condemns the use by Tucker and Vedlitz (1986) of a similar indicator on the grounds that underlying party bases probably did not remain stable during the span (1932–80) over which they

TABLE 7. State-by-State Regression Results for the South, 1966–94

State with Average Democratic Vote Percentage	Turnout	Democratic Incumbent	Republican Incumbent	Adjusted R^2	Number of Cases
Virginia	98.50x	—	−31.52	.45	11
48.56	(53.29)		(9.82)		
Louisiana	66.16x	0.21	—	.09	6
56.30	(44.34)	(5.54)			
Texas	12.53	4.30	−7.47	.12	19
49.62	(23.50)	(4.74)	(5.19)		
Georgia	−12.55	15.36	—	.33	17
62.40	(37.56)	(5.33)			
North Carolina	−13.23	7.03	−6.48	.32	11
52.00	(18.39)	(3.66)	(3.32)		
Mississippi	−30.04	4.63	−20.61	.51	11
53.25	(41.49)	(6.52)	(6.54)		
Tennessee	−32.79x	9.06	−7.96	.51	17
49.61	(26.86)	(4.47)	(4.23)		
South Carolina	−33.72x	13.55	−16.75	.79	19
50.72	(27.51)	(3.71)	(3.46)		
Florida	−34.56x	9.88	—	.49	16
54.47	(24.50)	(2.85)			
Arkansas	−51.02x	2.26	−9.24	.08	21
62.31	(32.55)	(4.67)	(11.23)		
Alabama	−67.58*	10.21	−7.55	.41	16
59.47	(28.76)	(4.45)	(6.46)		

Note: Estimates are by ordinary least-squares. The dependent variable is the *Democratic Percentage of the Vote*, with a range from 0 to 100. *Turnout* is measured as a ratio and ranges from 0 to 1. Cell entries are unstandardized regression coefficients with standard errors in parentheses.

and two-effects models to the four questions posed above.

The Shift after 1964

Why was the turnout effect outside the South strongly positive in 1928–64 but insignificant and negative in 1965–94? The conventional theory has a hard time explaining this most basic result. Using our extension of the theory, one might argue that the extent of class voting in the United States declined after 1964 due to the increased salience of noneconomic conflicts (civil rights, the Vietnam War, and social-cultural issues), with the result that fewer peripheral (low-SES) voters were inclined to the Democrats. But did the peripherality ratio fall so near unity as to wipe out the turnout effect altogether, as our data require? In the analysis of state-by-state patterns below, we find evidence that it did. In the meantime, we note that if one posits a total collapse of class voting to explain the end of the turnout effect after 1964, then one preserves the conventional theory (or at least DeNardo's formalization of it) at the cost of admitting that its principal prediction has not been true for thirty years. Still, such a resolution may be desired by some advocates of this theory, for it enables them to argue that the turnout effect will return if Democratic strategists (or exogenous events) sufficiently raise the political salience of class-linked economic issues.[21]

DeNardo has already applied the two-effects model to explain why the turnout effect vanished after 1964. In his original paper (1980), he noticed that turnout effects for House elections changed in 1962 and 1966. In a later analysis (1987), he showed that data from U.S. national elections between 1964 and 1980 in conjunction with the two-effects model imply that the core vote (both Democratic and Republican) shrank by approximately 50% during those years. In the model, as we noted above, a shrinking core produces a shrinking turnout effect. This derivation is consistent with an independent analysis of survey data on partisan self-identification by Converse (1976), who concludes that 1964 was the last election year preceding an "abrupt" partisan dealignment.[22] Our

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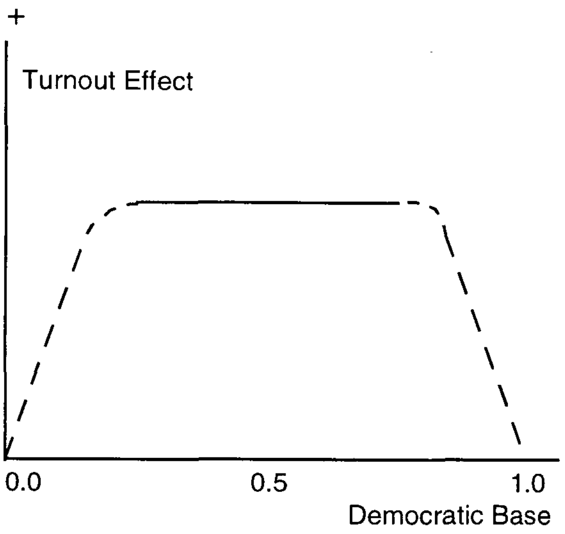
average the Democratic vote. We use shorter periods and avoid averaging across the 1965 dealignment divide. For another problem with the indicator, see footnote 26.

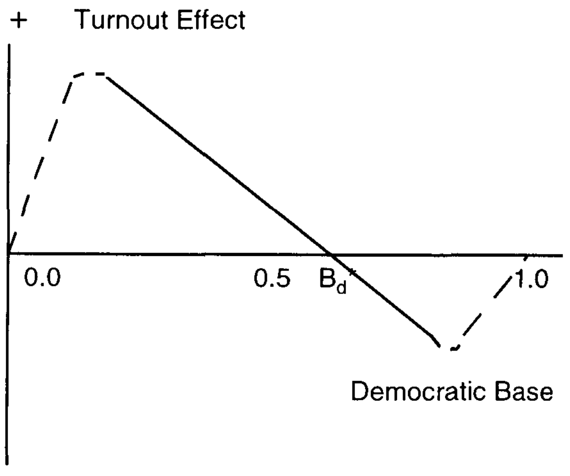
[21] In other words, when politics is dominated by a single (socioeconomic) dimension, cores are large, because voters are relatively free from cross-pressures and are consequently reliable voters and loyal partisans. When politics becomes multidimensional, core coalitions are harder to hold together, and peripheries become dominant. As a result, candidates shift from the politics of mobilizing dependable bases (with an emphasis on grassroots organization) to the politics of converting wafflers (mainly through mass media).

[22] Quoted by DeNardo (1987, 445). See also Carmines and Stimson (1989) for evidence of how the race issue fundamentally affected partisan loyalties beginning in 1964, and Aldrich (1995, chapter 8), for a compelling argument that the 1960s constituted a critical era in U.S. politics.

FIGURE 2. Predicted Relations Between Turnout Effect and the Democratic Base



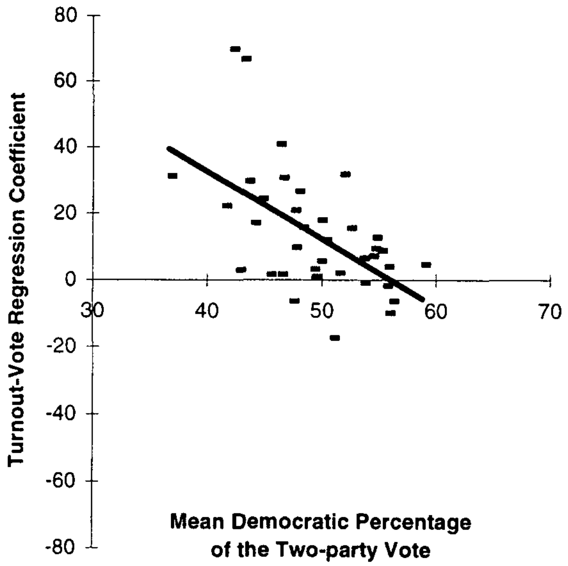
2a. Conventional Theory



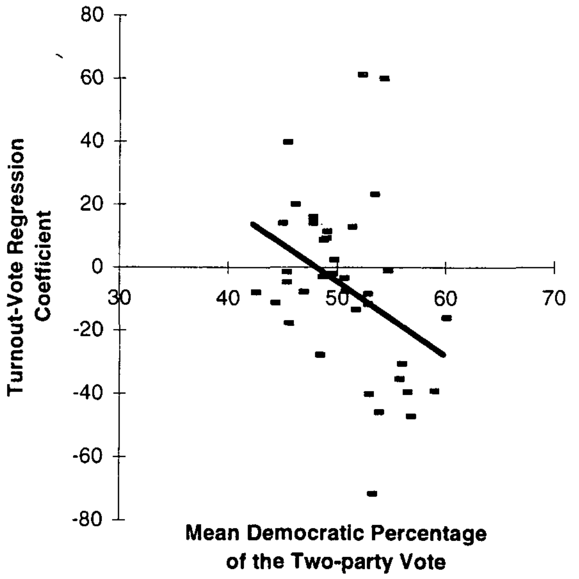
2b. Two-Effects Theory

---------- Outside the normal range of major statewide elections

FIGURE 3. Effect of Turnout on the Vote by Mean Percentage Democratic of the Two-party Vote, Nonsouthern States



1928-64



1965-94

finding of a shift after 1964 thus corroborates earlier work by Converse and DeNardo and provides new evidence consistent with the latter's model. We now proceed to ask whether his theory can also account for the finer details of our results.

Why Is the South Different?

As we noted earlier, the turnout effect for the South during 1966–94 was significantly negative, whereas the effect for the non-South in the same period was negative but small and insignificant. We cannot think of any way to explain these findings using the conventional theory, which implies that both coefficients should be positive. DeNardo's two-effects theory predicts that negative coefficients will appear when the Democratic base is more than a majority—how much more depends on parameters in the model that we cannot directly observe using the data in this study. As it happens, the mean Democratic vote in the South during 1966–94 was 54.6%;

outside the South, Democratic candidates averaged only 50.4% of the vote during the same years. Although we cannot say that DeNardo's theory strictly predicts this pattern, his model is at least consistent with a coefficient insignificantly different from zero in districts where the vote is almost evenly split and a clearly negative coefficient in districts where the Democrats have a definite majority.

We also found, however, that the southern turnout coefficient became positive in 1990–94. Can DeNardo explain these shifting signs within a single region? From 1966 through 1989, the mean Democratic vote in the South was 56%, consistent with a negative turnout effect in the two-effects model. During 1990–94, the Democrats lost their majority in southern senatorial and gubernatorial elections; the mean Democratic vote fell to 49.4%. Simultaneously, the turnout effect became positive, as DeNardo predicts it will be if Democrats are in the minority.[23]

Accounting for Variation across States

As Figure 2a shows, the conventional theory predicts a constant turnout effect regardless of the relative size of party bases, unless one party has an overwhelming majority. Although random differences are to be expected, it is difficult to reconcile this prediction with the wide variation in state turnout coefficients displayed in Table 5, nor does the theory explain in any obvious way why some states have significant turnout effects and others do not (although we shall explore shortly a possible nonobvious connection).

As represented by Figure 2b, DeNardo's model predicts that the relation across states between turnout coefficients (C) and average Democratic vote shares (B_d) should be of the form $C = aB_d + b$, where $a < 0, b > 0$, and $C = 0$ at $B_d > .50$. We can easily test these predictions for our two periods by regressing turnout coefficients for the 37 states on their mean Democratic vote shares. The estimated equation for 1928–64 is

$$C = -2.03B_d + 113.58 \qquad \textbf{(2)}$$

$$(\text{std. error} = .51, p < .001, r^2 = 0.31).$$

The equation for 1965–94 is

$$C\ -2.36B_d + 113.23 \qquad \textbf{(3)}$$

$$(\text{std. error} = 1.02, p < .03, r^2 = 0.13)^{24}$$

These relations are portrayed graphically, along with scatterplots of the data, in Figure 3. Despite the obvious differences between the two arrays in Table 4, these estimates are remarkably similar. Each has the signs that DeNardo's model predicts for both slope and intercept. Moreover, the estimate for 1928–64 (equation 1) yields a zero turnout effect when the average Democratic vote is 56%—a result impressively consistent with the pattern implied by the two-effects model.

The zero turnout solution for 1965–94 (equation 3) provides the only apparent setback for DeNardo's theory; the regression line crosses the axis at an average Democratic vote of 48%.[25] One might discount this discrepancy because the state estimates for 1965–94 are weaker and less reliable than those for 1928–64.[26] In DeNardo's model, however, an intercept near 50% does occur in the limiting case in which (contrary to the conventional hypothesis) the two parties attract equal proportions of peripheral supporters ($P_d = P_r$). If we accommodate the evidence to the model in this manner, then our estimate implies that the disappearance of turnout effects after 1964 is due not only to the shrinking of parties' core votes (as argued by DeNardo 1987) but also to dealignment of peripheral voters away from their previous Democratic inclination.[27] As we suggested earlier in discussing the overall difference between the two periods, such a dealignment of the periphery may reflect a decline in class voting resulting from the lower salience of economic issues since 1964. If this interpretation is correct, the defection mechanism has been more durable than the composition mechanism on which the conventional hypothesis depends.

The Effect of Party Organization

In our extension of the conventional theory, we suggested that the positive turnout effect it predicts may be negated not only by a decline in class voting but also by a party's mobilization of low-SES voters. According to this argument, if low- and high-SES voters turn out at nearly equal rates, then the Democratic peripherality rate will be no greater than the Republican. Having mobilized their working-class base, Democrats will generally do well, but fluctuations in turnout will have little effect on the vote, because effective mobilization also means that turnout will not vary much.

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[23] A similar argument may explain the anomalously low turnout effect outside the South in 1931–40 (Figure 1 and footnote 11 above). During that decade Democratic candidates averaged 52.1% of the vote, compared with only 44.2% in 1928–30 and 48.7% in 1941–65. However, DeNardo's theory also predicts lower turnout effects when turnout rates are high, as they were in 1931–40–64.1%, compared with 55.0% in 1928–30 and 58.6% in 1941–65. Thus the 1930s dip in the turnout effect may be due to both a large Democratic base and high turnout.

[24] Although a one-tailed test would be appropriate here, p values were obtained by a conservative two-tailed test.

[25] We also estimated a similar equation for the eleven southern states in 1966–94:

$$C = -3.94B_d + 205.58 \ (\text{std. error} = 165.58, p = .23, r^2 = .16),$$

which yields a zero turnout effect at $B_d = 52.1\%$. These results are roughly consistent with the pattern for nonsouthern states in the same period, except that the southern regression line crosses the axis above 50%. By the argument that follows in the text, Democrats in the South may have a slightly larger share of peripheral supporters than do nonsouthern Democrats.

[26] In personal correspondence with the authors (October 20, 1995), DeNardo points out another possibility: The mean Democratic vote share is generally a biased estimator of B_d. When there are defections from each party's base, the estimator will be biased toward 50%, so it can happen that a mean Democratic vote of 50% may correspond to B_d > 50%.

[27] If the core disappears entirely, the peripherality rates are necessarily equal at 1.0; but DeNardo (1987, 451) estimates that the core leveled off after 1980 at about 15–20% of those eligible to vote (compared with 40–45% before 1964).

The United States has not developed a socialist or labor party, but in some places traditional patronage-based party machines have served the same function of mobilizing people with lower SES. In his study of state parties, Mayhew (1986, 196) scored every state on a scale of traditional party organization (TPO) that ranged from 1 (weakest TPO) to 5 (strongest TPO). He defined TPO as requiring five characteristics: substantial autonomy, a long life span, hierarchy, influence over nominations for office, and reliance on material incentives. The scores were Mayhew's judgments based on his reading of the academic and journalistic literature on local parties in each state until the late 1960s, when most machines went into terminal decline (Mayhew 1986, 7–8, 17–23). Of the 37 nonsouthern states listed on the left-hand side of Table 4, 13 had TPO scores of 4 or 5—Connecticut, Delaware, Illinois, Indiana, Kentucky, Maryland, Missouri, New Jersey, New York, Ohio, Pennsylvania, Rhode Island, and West Virginia. The other 24 had TPO scores of 1, except New Mexico, which rated a 2.

A quick look at Table 4 suggests that the party-organization hypothesis may be worth testing. At the bottom of the list for 1928–64 is Illinois, the epitome of a TPO state, with the only negative turnout coefficient that approaches significance. The next most negative effects are for Rhode Island and New York, also strong TPO states. At the top of the list, the nine states with significantly positive turnout coefficients all had TPO scores of 1, except Pennsylvania (the Philadelphia machine was Republican until the early 1950s). Therefore, we decided to regress the state turnout coefficients against both TPO and the mean Democratic vote share during 1928–64, which is our surrogate for DeNardo's chief variable, B_d, the Democratic base. As a predictor of turnout effects, the mean Democratic vote survived this test with flying colors. Its effect on the turnout regression coefficients remained highly significant and declined only a little in magnitude compared with the bivariate result in equation 2. TPO had the predicted negative sign (that is, strong TPO tended to be associated with a reduced effect of turnout on the Democratic vote), but its coefficient fell short of statistical significance ($t = -1.26, p = .22$).[28]

We conclude this section with an observation that emerged serendipitously from our inquiry into the effects of party organization. Our data reveal that the well-known decline in turnout after the early 1960s was heavily concentrated in formerly strong TPO states. Between the two periods (1928–64 and 1965–94), average turnout in the 24 low-TPO states fell by 7%, from 59% to 52%; but turnout in the 13 high-TPO states dropped twice as much, from 61% to 47%. Except for Maryland, where turnout fell 7.2% (perhaps buoyed by government workers), all the TPO states experienced turnout declines far above the average for non-TPO states. Some of the decreases were staggering: 17.8% in Delaware, 18.5% in New York, 19.4% in Indiana, and 21.6% in West Virginia. In short, although it cannot be the whole story, the collapse of TPOs appears to account for an important part of the decline in U.S. turnout since the 1960s.

DISCUSSION

We now offer a few reflections about what our study implies for politicians and political scientists as well as what it does *not* imply for people with egalitarian concerns about the quality of democracy in the United States.

The conventional wisdom cherished by so many politicians *was* true for the United States as a whole during a long period. Even in its heyday, however, the turnout effect varied across space and time according to the underlying partisan composition of the electorate. Ironically, it helped Democrats most where (and when) they were less numerous, and if they were too popular it vanished or even turned in favor of Republicans. In the modern era, the effect of turnout on partisan success has remained variable in this way, but with the shrinking of partisan cores and the dealignment of peripheral voters, the effect has become generally small and unreliable—insignificant statistically and politically. The evidence of the last three decades suggests that politicians of both parties can relinquish their hopes or fears about higher voter participation. At worst, they can focus their efforts on facilitating or hindering voting by specific groups whose partisanship is predictable.[29] At best, they are free to judge policies governing elections according to what is best for the polity, rather than what is advantageous for the party.

For political scientists, our findings suggest the value of paying close attention to DeNardo's model. We did not begin this project as proponents of his theory. Indeed, its complex predictions had no influence on our initial research design. We turned to the two-effects model only when it became apparent that neither the conventional theory nor its simple negation could handle the varied and unexpected patterns we uncovered. Such independence between theory and tests may not be textbook scientific procedure, but it makes the consistency between our results and DeNardo's predictions all the more impressive. We believe that our findings should establish the two-effects model as the best available explanation of turnout-vote relationships in the United States and as the starting point for further investigations into the subject.

Some may be reluctant to accept this conclusion, not only because of the nonintuitive complexity of the two-effects theory but also because of the hope the conventional theory inspires that increasing voter turnout may be an obvious practical way to produce desired changes in U.S. politics. For those with such aspirations, it may be helpful if we comment briefly about what our argument does *not* imply about substantive political outcomes.

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[28] A multistage path model shows that party organization influences the effect of turnout on the vote mainly by stabilizing parties' vote shares. (TPO significantly depressed the standard deviation of the Democratic vote, which in turn had a significant positive effect on turnout coefficients.) Strong TPO also increased the mean Democratic vote share.

[29] Wolfinger (1993) shows that much of the maneuvering over the motor voter bill revolved around such calculations, but the predisposition of most Republicans was to believe they benefited from light turnout.

Even more than most statistical evidence, the data on turnout effects tell us about the past, not the future. With respect to the immediate question of partisan advantage, we claim only that party fortunes have not been consistently affected by the fluctuations in voting participation during the low-turnout era of the past three decades. In an earlier period, higher turnout was, on the whole, associated with a larger vote share for Democrats. Although extrapolation from the recent past is probably a safer bet, what was once true could become true again. DeNardo's model points to crucial intervening variables that explain the presence or absence of turnout effects—the relative proportion of reliable (core) voters and the strength of partisan identification among irregular (peripheral) voters. The model per se does not explain why the core has shrunk and the periphery has become less Democratic, but scholars and activists who hope for a revival of turnout effects might profitably direct their attention to strategies that will influence those variables.

Most advocates of higher turnout ultimately care more about policy outcomes than partisan advantage. They believe that a shrunken, skewed electorate fostered inegalitarian economic policies during the 1980s and 1990s. Here, again, the two-effects model and the data on turnout effects are agnostic. If political parties and candidates adapt their positions to the interests of probable voters, however, then an expanded, more representative electorate may induce both major parties to shift their policies in a more egalitarian direction, quite independently of whether short-term turnout fluctuations favor Democrats or Republicans.[30]

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[30] Among others, Edsall (1984, chapter 5), Piven and Cloward (1988, 253–5), Rosenstone and Hansen (1993, 241–8), and Verba et al. (1993) have all made arguments consistent with this point.

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Do Majority-Minority Districts Maximize Substantive Black Representation in Congress?

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Majority-minority voting districts have been advanced as a remedy to the underrepresentation of minority interests in the political process. Yet, their efficacy in furthering the substantive goals of minority constituents has been questioned because they may dilute minority influence in surrounding areas and lead to an overall decrease in support for minority-sponsored legislation. Thus, there may be a trade-off between increasing the number of minority officeholders and enacting legislation that furthers the interests of the minority community. Using nonlinear estimation techniques, we simulate the districting strategies that maximize substantive minority representation, and find that such a trade-off does exist. We also find that, outside of the South, dividing minority voters equally across districts maximizes substantive representation; inside the South the optimal scheme creates concentrated districts on the order of 47% black voting age population. In addition, minority candidates may have a substantial chance of being elected from districts with less than 50% minority voters.

After the 1990 Census, the North Carolina state legislature drew up a reapportionment plan whereby only one district out of twelve had a majority of minority voters, that is, was a "majority-minority" district. This plan was then submitted to the Department of Justice under the preclearance procedures of Section 5 of the 1965 Voting Rights Act. Upon review, the Justice Department rejected the proposal, suggesting that the state construct a second such district to accommodate its 20% black population. North Carolina's second attempt did result in two majority black districts, the First and the Twelfth, but the latter was, to put it mildly, rather unusually shaped; it snaked along Interstate 85, occasionally ballooning out to capture pockets of black residents and, at times, remaining contiguous only at a single point. The Supreme Court reviewed the second redistricting plan in *Shaw* v. *Reno* and ruled that bizarrely shaped majority-minority districts may create unconstitutional racial gerrymanders.[1] Coming three weeks after the withdrawal of Lani Guinier's nomination to head the Civil Rights Division of the Justice Department, the *Shaw* decision intensified the debate over the role of majority-minority districts in promoting black representation in Congress.

The past quarter-century has seen the rise of what Guinier (1994) terms the "theory of black electoral success," according to which the advancement of minority interests can be measured by the number of minorities elected to public office. This goal has been achieved largely by the construction of concentrated minority voting districts, either through the decennial reapportionment or by switching from at-large voting to district-based systems in local elections. Proponents of these districts argue that, given polarized voting, minorities will remain underrepresented in the political process unless they have equal opportunity to elect the candidate of their choice. If this is true, and if minority voters generally elect minority candidates to represent them, then *descriptive representation*—increasing the number of minority officeholders—goes hand in hand with *substantive representation*—enacting legislation that furthers the interests of the minority community.

Yet, it is unclear that minority interests are always best served by the creation of concentrated minority districts. These dilute minority influence in surrounding areas, which may then elect representatives unsympathetic to minority concerns. If minority voters can influence their representative's actions without necessarily comprising a majority of the electorate, then majority-minority districts may increase the number of minority legislators but decrease the number of votes in support of minority legislation. That is, there may be a trade-off between descriptive and substantive representation. Thus, a basic question of constituency and governance remains unresolved: Do majority-minority districts maximize substantive black representation in Congress? Is it better for political minorities to wield a modest amount of influence in many districts or substantial influence in only a few?

To address this question we develop a general methodology for assessing the effect of different districting schemes on the substantive representation of group interests, as measured by legislators' roll-call voting

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[1] *Shaw* v. *Reno*, 113 S.Ct. 2816 (1993). The decision remanded the case to the federal district court to review the reapportionment plan under strict scrutiny. The lower court eventually upheld the North Carolina district on the basis that it united urban interests as well as racial minorities, but the Supreme Court again ruled against the district in *Shaw* v. *Hunt* (1996).

scores. We then apply this technique to calculate the districting strategy that maximizes black representation in Congress. Our approach consists of three steps. First, we estimate *representation equations*, which relate the black voting-age population in a district to the representative's support for minority issues. Second, we estimate *electoral equations*, which relate the concentration of minority voters in a district to the partisan and racial characteristics of the representative elected. Third, we combine the representation and electoral effects to calculate the districting scheme that maximizes overall expected representation of minority interests.

Briefly, our results are as follows. First, a trade-off does exist between maximizing the number of black representatives in Congress and maximizing the number of votes in favor of minority-sponsored legislation. In particular, districting plans designed to maximize descriptive representation concentrate minority voters more than do plans designed to maximize substantive representation. Second, electoral effects dominate representation effects; that is, the largest effect from adding minority voters to a district comes not from influencing the actions of any given representative but from influencing the type of representative elected. Third, the point of equal opportunity for minority voters to elect the representative of their choice generally occurs in districts with less than 50% black voting-age population. Fourth, given recent electoral and roll-call voting patterns, the districting strategy that maximizes substantive minority representation varies by region. Outside the South, optimal districting schemes divide black voters as equally as possible across districts. Inside the South, substantive minority representation is maximized by creating concentrated minority districts with about 47% black voting-age population.

The rest of the paper is organized as follows. In the next section we review the literature on minority representation. In the third section we present the data used in the study, detail our estimation techniques, and analyze the representation and electoral equations. In the fourth section we conduct a series of simulations of the optimal allocation of minorities to districts that maximizes their influence in Congress. We conclude by relating our findings to the larger debate on minority representation.

THE DEBATE ON MAJORITY-MINORITY DISTRICTS

Two distinct literatures discuss the effect of minority voting strength on a representative's responsiveness to minority interests. The first focuses on how an increase in the minority voting population translates into discernible policy gains. Its emphasis is on finding the appropriate functional relationship between the percentage of black voting-age population in a district and the legislator's roll-call voting behavior. The second literature concentrates on the effect of majority-minority districts in promoting descriptive and substantive representation of black interests. Although these literatures have sometimes been treated separately, we contend that answering the first question—how district composition translates into legislative behavior—is crucial in assessing the influence of majority-minority districts in promoting black interests. We review these literatures and then discuss how they can be integrated to give a broader perspective on the efficacy of majority-minority districts.

Minority Interests and Representation

In single-member district plurality-winner elections, what influence will electoral minorities have over the actions of their representative? A number of different answers have been advanced in the literature. If preferences within the electorate are polarized, with one group commanding a clear majority, then electoral minorities will have only a tenuous relation with their representative. If the majority is itself divided, then these minorities may have a good deal of influence over outcomes by trading their support in return for policy concessions. The first scenario corresponds to the situation of most blacks in southern politics since Reconstruction; the second reflects the position of blacks as key swing voters in national politics from the late 1950s to the mid-1960s.

We thus begin with two hypotheses about the relation between the percentage of black voters in a district and the behavior of their representative. The first, which we term *majoritarianism*, predicts that black voters in a district will have little influence on the voting behavior of their representative until they constitute a majority, at which point the representative's voting behavior takes a discrete jump toward minority-favored policies. The second, termed *influence districts*, posits a generally positive relation between the percentage of blacks and representation of black interests, as would most standard theories of interest group behavior.

Other possibilities have been mentioned in previous studies of race and representation. Historically, the most important of these was suggested by Key (1949) in his classic *Southern Politics*. Key notes that those counties with the highest proportion of blacks were the most likely to vote for Smith over Hoover in 1928 and to bolt the party in favor of Strom Thurmond's State's Rights ticket in 1948. He also suggests that members representing these same districts compiled relatively more conservative voting records. Thus, we might actually expect a *negative* relation between the percentage of blacks and support for minority legislation. The argument is that in a polarized district with a relatively large black population, minority issues become more salient. Representatives from these areas will feel pressured by conservative white constituents to prove that they have not been unduly influenced by black voters. We shall call this possibility the *polarization* hypothesis.[2]

A variant of Key's hypothesis comes from Keech (1968), who posits a *curvilinear*, or bimodal, relationship between the percentage of blacks and their representative's voting patterns. Keech asserts that the majority does not take much notice when the percentage of

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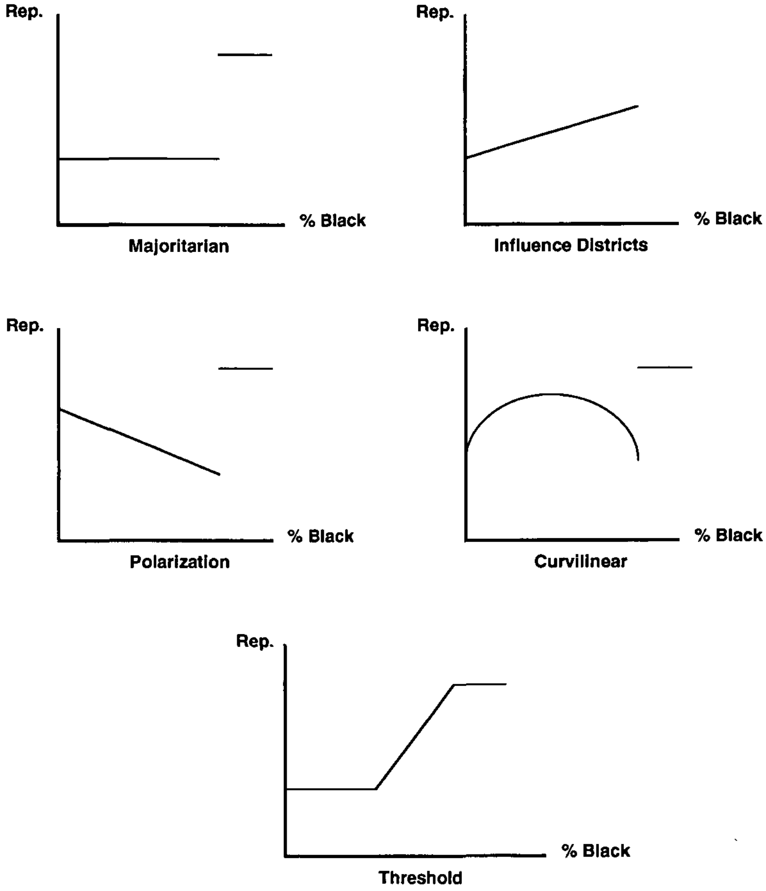
[2] Additional evidence of the polarization effect is demonstrated by Kernell (1973), who found that in Mississippi counties white voter turnout is highly correlated with the potential and actual black turnout. McDonald (1992, 81-2) provides additional examples of white backlash to minority mobilization.

FIGURE 1. Hypotheses Concerning the Relation between Percentage of Black Voters and Representation of Black Interests



blacks in a district is fairly low, but once it reaches a critical level, say, 20–30%, the polarization effect takes over, and the representation of minority interests will be flat or actually decline until blacks comprise a majority of the voting population.

Finally, we consider the possibility of a *threshold* effect, that is, blacks will have no influence until they reach a minimum level of strength, after which a rising relationship to the representative's voting pattern is observed. Recent theoretical research (Epstein and O'Halloran 1995) derives this possibility from a multiple-principals model of representation. Indeed, McClain and Stewart (1995, 25) point to this possibility in their discussion of influence districts: "Surely there is a threshold of racial and ethnic minority representation necessary to have the interests of these groups represented at all."

These five theoretical models of minority representation are shown schematically in Figure 1. One purpose of our project, then, is to estimate the relation between minority voters and the representation of black interests in the modern Congress, to see which hypothesis, if any, best fits the data.

A number of empirical studies since Key's time have investigated the nature of the relationship between minority population and their representative's roll-call voting patterns with decidedly mixed results. For convenience, Table 1 summarizes these findings. Black (1978), analyzing southern legislators' support for voting rights legislation, found that until 1970 the Key polarization effect was in evidence, as those districts with the highest proportion of blacks tended to be far more conservative than those with less than 20% blacks. Over time, however, as more blacks entered the political process, overall southern opposition to civil rights legislation waned, and the relation between the percentage of blacks and voting scores came to resemble Keech's curvilinear hypothesis. Similarly, Bullock (1981) analyzed legislators' mean support scores categorized by the proportion of blacks in their district and found some support for Keech's hypothesis, although this pattern at times appeared to be bimodal, with peaks around 20–30% black.

Combs, Hibbing, and Welch (1984), who disaggregated members by region, party, and the percentage of blacks, also found a pattern akin to Bullock's bimodal relationship. The authors argued, however, that this relationship results from an interactive effect between urbanization and the percentage of blacks. When the data were further decomposed by the percentage of urban population, the authors uncovered the Key polarization effect in the rural South and a positive influence

TABLE 1. Empirical Studies on the Relationship between Percentage of Blacks and Legislative Support for Minority Issues

Study	Years	Dependent Variable(s)	Independent Variable(s)	Findings
Black (1978)	1957–75	Support for five voting rights acts	Percentage of blacks in district	Starts with Key's polarization relation and, over time, turns into Keech's curvilinear relation
Bullock (1981)	1959–78	Mean LCCR support and CC opposition score	Percentage of blacks, party, generational replacement, and deep South	Both Keech curvilinear and bimodal relationships
Combs, Hibbing and Welch (1984)	1973–80	CC support scores	Percentage of blacks, party, race of representative, region, urban, and foreign stock	Bimodal relationship, which decomposes into influence districts for urban areas and polarization for rural areas in the South; also, influence districts in the North
Whitby (1985)	1969–82	LCCR score	Percentage of blacks, urban, party, median education, and generational replacement	Percentage of blacks not significant, but the interaction between black and urban is significant
Whitby (1987)	1983–84	ADA, COPE, LCCR, ACA, CC, BI scores	Percentage of blacks, urban, party, income	Percentage of blacks not significant, but the interaction between black and urban is significant
Lublin (1994)	1972–90	Nominate scores	Percentage of blacks, region, urban, party, education, and income	Percentage of blacks significant, with a structural break at 40% black population; no support for quadratic or cubic relation

Note: LCCR = Leadership Conference on Civil Rights; CC = Conservative Coalition; ADA = Americans for Democratic Action; COPE = Committee on Political Education; ACA = Americans for Constitutional Action; BI = Black Index, defined by author; Nominate scores = constructed index from Poole and Rosenthal (1991).

district effect in the urban South and the North.[3] Whitby (1985, 1987) again found that the interaction between black and urban proportions is an important determinant of representatives' voting scores. Lublin (1994) rejected nonlinear estimates of the relation between the percentage of blacks and a representative's voting behavior in favor of a pattern of linear influence districts, with a structural break at 40% black population.[4]

Thus, statistical analyses of voting behavior offer no clear picture of the relationship between constituent characteristics and the representative's actions. Swain (1993) took a different approach to the question, interviewing all black members of the U.S. House (as of 1990) as well as white representatives of districts with a significant proportion of black voters to discover their positions on racial issues and governance. She argues that black representatives can win election outside majority-black districts by emphasizing issues important to their broad constituency and that white representatives will advance some of the issues important to their black voters. Swain concluded that a majority-minority districting strategy has only limited possibilities and that multiracial districts offer the greatest avenue for advancing minority political interests. Her larger project was to argue that the future representation of minority interests should not be associated exclusively with the creation of majority-minority districts. As such, her research serves as a bridge between the studies that link voting behavior to the minority percentage in the electorate and the literature that focuses on the importance of minority-controlled electoral districts.

The Efficacy of Majority-Minority Districts

Swain's analysis is the latest chapter in a long debate over majority-black districts and the importance of electing minorities to office (for overviews of this literature, see Davidson 1984 and Grofman and Davidson 1992). One side in this debate contends that the election of minority candidates is a necessary intermediary step, both symbolically and substantively, toward achieving racial political equality. Unless minorities have the power to elect candidates of their choice, the argument goes, their votes will be submerged in a sea of unsympathetic majorities. For instance, Davidson (1992, 46–7) argues: "As the political history of the southern states in the present era makes clear, whites have been quick to use the entire panoply of dilutionary measures to maintain their advantage in officeholding. . . . Fairness alone dictates that the [Voting Rights] Act protect the ability of recently enfranchised minority voters to elect their candidates" (see also Cain 1992, Kousser 1993).

This argument is borne out in some of the earliest statistical studies linking black officeholding to concrete policy gains. Keech (1968, 93) asserted that black political participation affected both electoral outcomes and the distribution of services in Durham, North Carolina, and Tuskegee, Alabama. Yatrakis (1981) concluded that the black mayor of Newark, New Jersey, brought significant policy benefits to the black community. And Browning, Marshall, and Tabb (1984, 141) similarly found that the presence of minorities on local councils reduces polarization and stereotyping and also leads to "the creation of police review boards, the appointments of more minorities to commissions, the increasing use of minority contractors, and a general increase in the number of programs oriented to minorities" (for a further discussion of recent policy gains associated with black political participation and officeholding, see McDonald 1992).

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[3] Southern urbanization also figured prominently in Key's analysis: "The growth of cities contains the seeds of political change for the South" (1949, 673).

[4] Recently, Overby and Cosgrove (1996) found a significant relation between members' COPE scores and the change in their district's black population from 1992 to 1993.

Increasingly, however, voices from within and without the civil rights community have begun to doubt the efficacy of majority-minority districts in securing minority representation in the current legal and political climate. First, majority-minority districts rely on segregated housing patterns and are therefore less effective at uniting a widely dispersed minority, especially after the *Shaw* decision. Second, there is a fear that these districts may politically "ghettoize" minorities, limiting their influence to a few safe black districts while marginalizing the minority vote elsewhere. This may result in the election of racially conservative candidates in other areas, offsetting the gains from the safe minority district.[5]

For instance, Brace, Grofman, and Handley (1987) found a positive and significant correlation between the number of majority-minority seats created in proposed South Carolina redistricting plans and the expected number of Republicans elected. More recently, Hill (1995) concluded that majority-minority districts cost the Democratic Party about six seats in the 1994 congressional elections. McDonald (1992, 81) notes that "increased minority participation has in turn caused an exodus of conservative white Democrats to the Republican party. If the exodus continues, some have argued, the result could be an increasingly black but politically impotent Democratic party opposed by an increasingly white and dominant Republican party pursuing ever more racially regressive policies."[6] Lublin (1994, 245) concluded that "efforts to maximize the number of black elected members of the House and pro-black congressional legislation tend to work at cross-purposes." Finally, it has often been noted that majority-minority districts may hinder the formation of the biracial liberal coalitions that were the impetus behind the original civil rights campaign.

The overall efficacy of majority-minority districts in advancing black interests, therefore, remains unresolved. These districts certainly increase the number of minority candidates elected to office, that is, the descriptive representation of minorities.[7] But it is unclear that concentrated minority districts augment the substantive representation of minorities or the chance that legislation favored by the minority community will be enacted by Congress.

Majority-Minority Districts and the Representation of Minority Interests

We have reviewed two literatures, one relating the percentage of black voters in a district to their representative's voting behavior and the other debating the benefits of majority-minority districts. Although we have treated them separately, it is clear that they are intimately related. Assume, for instance, that the relationship between the percentage of blacks and support for legislation favorable to minorities is exactly linear, either increasing or decreasing. It then makes no difference how minorities are distributed throughout districts, as the average level of support will be the same in all cases.[8] If a minority group is underrepresented unless it comprises a majority in the jurisdiction, however, then the best way to ensure minority enfranchisement is to create as many majority-minority districts as possible. Finally, if partisan effects are the most significant feature in the political landscape, then reapportionment schemes which result in both minorities and Republicans being elected may decrease the overall average support for minority concerns.

Thus, in general, the representation relation and the optimal distribution of minority voters across districts are closely intertwined. To be more precise, an accurate measure of the former will allow us to calculate the configuration that gives minorities the greatest expected overall substantive representation. This may involve the construction of many concentrated minority districts, or few, or none. We investigate this question by first estimating the relationship between the percentage of black voters and legislators' representation scores. We then estimate the link between the black voting-age population and the probability of electing a black Democrat, nonblack Democrat, or a Republican to office in each of three geographic regions. Finally, we estimate the optimal apportionment of minority constituents in

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[5] Along these lines, there is a lingering suspicion that the alacrity with which the Justice Department under presidents Reagan and Bush supported minority districts was partially motivated by potential Republican gains in southern states. See Polsby and Popper (1993) for an entertaining discussion of why Republicans benefit disproportionately from the construction of concentrated minority districts.

[6] Similarly, Swain (1993, 205) quotes Craig Washington, a black U.S. House member from Texas; "If you have four districts in a state like Alabama, for example, with a sufficiently large black population to neutralize Republicans on some issues, and if you can create one black district by gathering up all the blacks, and in the process you lose the leverage that you had in the three other districts, then that's foolish to me. Every time the one person votes for the things that the black community is for, the other three will probably vote against them."

[7] See the essays in Davidson and Grofman (1994) for clear evidence that increased minority officeholding in the South is due almost entirely to the creation of majority black voting districts.

[8] Assume the relation between representation and percentage of black voters is $Rep = a + b * (\%black)$. Consider a state with n districts and P_b percentage of black voters. Then, total representation will be equal to $na + b * P_b$ and average representation $(a + b * P_b)/n$, no matter how minority voters are divided among districts. This implies that linear methods for calculating representation will not be effective for devising optimal districting strategies.

TABLE 2. Data Sources, Descriptive Statistics, and Coding Rules

Variable	Description	Mean	Minimum	Maximum	Source
LCCR	Member's civil rights voting record	59.6	7.7	100	Leadership Conference on Civil Rights, Civil Rights Voting Record for the 103rd Congress
MODCQ	Support for measures in which more than 60% of black representatives voted alike	54.68	4.0	100	*Congressional Quarterly*, Key Votes of the 103rd Congress
Party	1 for Republicans; 0 otherwise	0.41	0	1	Barone and Ujifusa, *Almanac of American Politics, 1994*
Race	Race of member: 1 for black; 0 otherwise	8.7%	0	1	*Congressional Quarterly*, vol. 52, supplemental to issue no. 44, p. 10
BVAP	Percentage of blacks of voting age in the district	10.96%	0.099%	72.14%	1990 Census data
Cover	1 if district is covered under Section 5 of the VRA; 0 otherwise	28.7%	0	1	Bott, *Handbook of U.S. Election Laws and Practices*, pp. 249–51 and 257–62
South[a]	1 for southern states; 0 otherwise	31.3%	0	1	*Congressional Quarterly Almanac*, 1994
East[b]	1 for eastern states; 0 otherwise	21.0%	0	1	*Congressional Quarterly Almanac*, 1994

Note: LCCR = Leadership Conference on Civil Rights; CQ = *Congressional Quarterly*, BVAP = black voting-age population; Cover = judicial oversight under the Voting Rights Act.

[a] Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, and Virginia.

[b] Connecticut, Delaware, Maine, Maryland, Massachusetts, New Hampshire, New Jersey, New York, Pennsylvania, Rhode Island, Vermont, and West Virginia.

each region that maximizes substantive representation in Congress.

THE DATA

Table 2 describes the data used in the analysis. Legislators' support for minority issues is measured by the Leadership Conference on Civil Rights index (LCCR) for the 103rd Congress, which is compiled from votes cast on 14 bills considered important to minority interests.[9] Since failure to vote lowers these scores, we have undertaken the standard procedure for attendance-correcting, eliminating from the total possible votes those measures on which a member did not actually cast a ballot. If a member was replaced through retirement or death, we calculated the average district support score.[10] The mean LCCR support score for all members was 59.6, ranging from a low of 7.7 to a high of 100.

Two comments are in order about our use of LCCR scores as a measure of minority representation. First, there is the issue mentioned above of descriptive as opposed to substantive representation. If descriptive representation is an overriding goal, then there can be no substitute for policies that favor the election of minorities to office, just as in the classic arguments for affirmative action. In practice, however, majority-minority districts have been neither sanctioned by the courts nor pursued by the Justice Department on the grounds of descriptive representation; the avowed goal has always been to increase the possibility that minorities can affect public policy by electing the candidate of their choice, regardless of race. There is also the "slippery-slope" argument that, once descriptive representation is acknowledged as a goal, it will be difficult to define exactly which groups should be afforded such an opportunity. Therefore, we investigate the effect of majority-minority districts on substantive representation and then examine the implications of the analysis for the number of minority candidates elected to office.

Second, the question arises as to whether the positions embodied in the LCCR votes are (1) merely liberal policies which are not actually helpful to the minority community and/or (2) not what the minority community actually wants, given a split in opinion between black elites and voters. Certain commentators have argued, for

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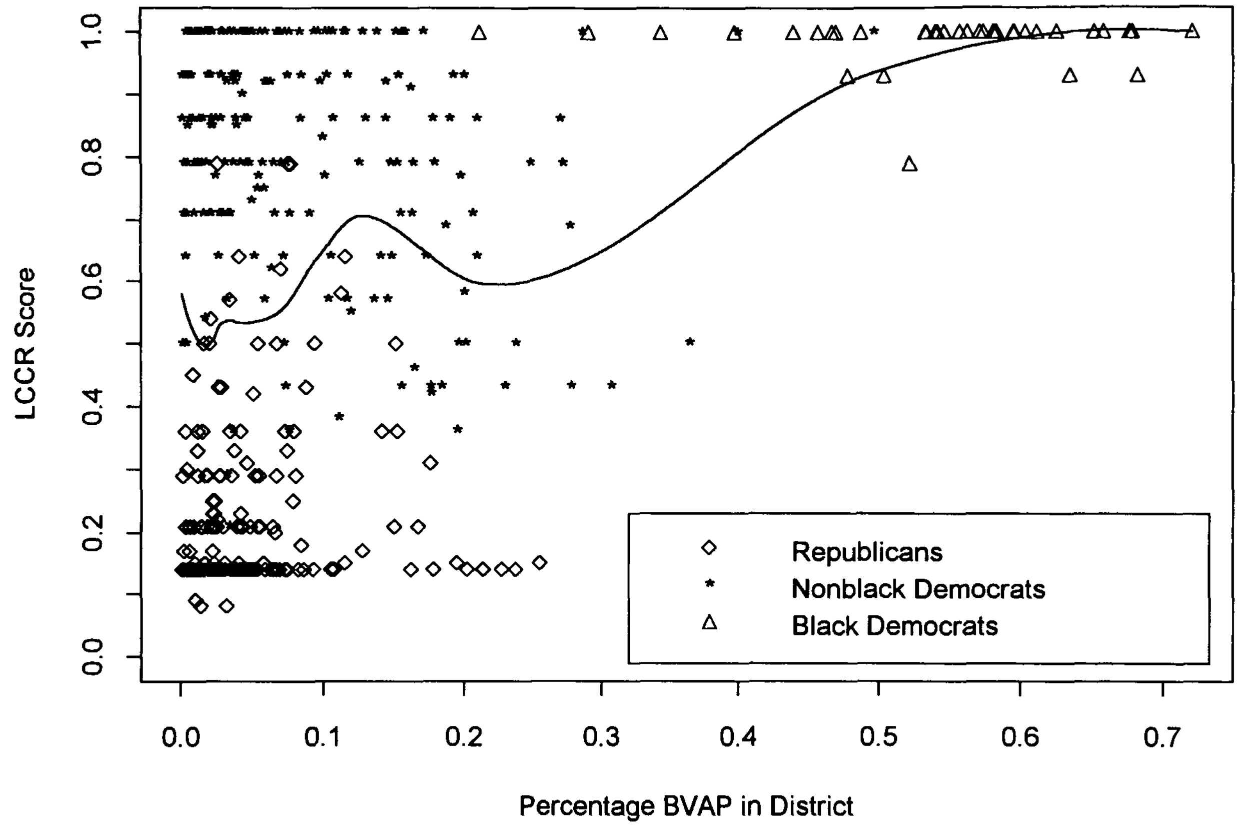
[9] Swain (1993) argues that the LCCR index captures only a member's position on civil rights issues and not overall support for redistributive policies that also may be important to black constituents. To account for this possibility, we constructed another measure, labeled modified Congressional Quarterly index (MODCQ), based on the *Congressional Quarterly* key votes for the 103rd Congress. We identified the position adopted by the majority of black representatives and coded whether a member voted with this majority. Issues that divided the Black Caucus—votes on which less than 60% of its members voted the same way—were eliminated from the sample. The votes included in both the LCCR and MODCQ indexes are provided in Appendix A. As the correlation between these indexes is 0.96, we use only LCCR scores in our analysis.

[10] Two districts changed partisan alignment in midsession. The roll calls from the first (Oklahoma 6th) were eliminated from the sample, although the election was used in calculating the electoral equations. In the second (Kentucky 2d), nine of the eleven votes were cast by Natcher (D) and only two were cast by Lewis (R). The district was coded as Democratic, and only those votes cast by Natcher were included in the analysis.

FIGURE 2. Relation between Black Voting Age Population and LCCR Voting Index Score



instance, that liberal programs have failed to secure economic advancement for minorities and that alternative policies might better serve these populations. We do not pretend to answer the questions of whether the policy positions characterized by these votes are "correct" or whether each of these votes corresponds with the underlying preferences of the minority community. We do assert, however, that whatever the substantive opinions of the minority community, they are likely to be closer to the policy positions taken by legislators in districts with a substantial population of minority voters as opposed to districts with fewer minority constituents. The high correlation between LCCR scores and the MODCQ index, which is constructed from votes actually cast by black legislators, therefore suggests that LCCR scores are a reasonable measure of substantive minority interests.

The key independent variable in the analysis is the black voting-age population (*BVAP*) in each district as a proportion of total population. We also record each representative's region: South, East, and other. A number of studies have tried to capture additional subregional distinctions by including a Deep South variable. A better proxy for historic discrimination against minorities is to control for whether a district is covered under the preclearance procedures of the 1965 Voting Rights Act, as amended.[11] The variable *Cover* codes which districts are subject to judicial oversight of any changes to electoral systems or reapportionment plans. In our sample, 125 districts are covered, including 95 out of 137 districts in the South.

Finally, we have recorded the race and partisan affiliation of each member. The 103rd Congress had 38 black members, all but one of them Democrats. The partisan division for the Congress was 256 Democrats, 177 Republicans, and one Independent.[12]

We first determined the appropriate level of aggregation for our analysis. Figure 2 plots BVAP against LCCR scores for all members of the 103rd Congress. Included in the figure is an extremely flexible, highly data-sensitive regression line, a local regression or "loess" line.[13] The loess line indicates a generally rising pattern, with some evidence of bimodality at 20–30% BVAP. The figure also clearly identifies three distinct populations in the data: black Democrats, confined almost exclusively to the upper right quadrant of the figure; nonblack Democrats, located primarily in the upper left quadrant;

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[11] In 1975, the Voting Rights Act was amended to include language minorities (mostly Hispanic) as well as racial minorities.

[12] Bernie Sanders (I-Vermont) is coded as a Democrat. The one black Republican in the 103rd Congress was Gary Franks, from Connecticut; his LCCR score of 0.21 was similar to that of other northeastern Republicans.

[13] Loess fitting, which we employ below, has become a workhorse of modern nonlinear, nonparametric regression analysis. See Hardle (1990), Fox (1991), Chambers and Hastie (1993), and Venables and Ripley (1994); Cleveland (1993, 93–101) provides an accessible introduction to loess fitting, including an overview of the computational details.

TABLE 3. Average Black Voting-Age Population and Leadership Conference on Civil Rights Scores by Subgroup

Subgroup	BVAP Mean	BVAP Median	LCCR Mean	LCCR Median	Number of Districts[a]
(1) Nonnortheastern Republicans	5.12%	3.70%	0.19	0.14	133
(2) Northeastern Republicans	5.22	3.94	0.35	0.35	42
(3) Southern nonblack Democrats from covered districts	14.49	15.31	0.62	0.57	47
(4) Southern nonblack Democrats from noncovered districts	10.20	7.33	0.71	0.71	21
(5) Nonblack Democrats from outside the South	6.24	3.72	0.90	0.93	154
(6) Black Democrats	54.25	55.79	0.99	1.00	37
All representatives	10.96%	4.87%	0.60	0.70	434

[a] The 6th District in Oklahoma changed partisan alignment in midsession and was eliminated from the sample. See footnote 10.

and Republicans, located predominately in the lower left quadrant.

The analysis that follows is therefore performed at the subnational level, allowing us to capture important regional and partisan variations in the data. A well-chosen set of subgroups will display fairly cohesive behavior within each group but different behavior across groups. Earlier studies, as well as Figure 2, indicate the importance of party, race, region, and coverage under Section 5 of the Voting Rights Act in predicting representatives' voting behavior. These factors suggest that the data be partitioned into the six subgroups shown in Table 3.

ESTIMATING REPRESENTATION AND ELECTORAL EFFECTS

In this section we estimate representation and electoral equations, which we will then combine to simulate optimal districting strategies. This two-step procedure allows us to capture the underlying trade-off between electing minority candidates and the overall representation of minority interests. It also allows us to calculate the effect of changing electoral conditions, such as a surge in Republican popularity, on policy outcomes.

Notice also that the estimations below are bivariate, including BVAP as an independent variable but excluding other socioeconomic indicators, such as education, income, and urbanization, that are often used as covariates in analyses of voting behavior. We employ bivariate analysis because the estimated relations will be used in simulations that implicitly reallocate black voters across districts within a state. But this redistricting will, on average, change levels of the other socioeconomic variables as well, both in the districts from which voters were taken and the districts to which they are given. A standard multivariate analysis of voting scores or electoral probabilities would thus misrepresent the effects of voter reallocations, since such an analysis would hold factors like urbanization constant as racial composition shifts, rather than let them vary, as does bivariate analysis.[14]

Representation Equations

We first estimate representation equations, which detail the effect of black voters on their representative's voting behavior. In particular, we calculate members' expected LCCR score given the level of BVAP in their district and the subgroup θ from which the representative is drawn:

$$E(LCCR \mid BVAP, \theta). \qquad (1)$$

The following method of analysis is employed in each of the six subgroups. First, we estimate general additive models using loess and smoothing splines, another flexible and potentially nonlinear, nonparametric regression method. Next, we employ an appropriate statistical test, an F test, to determine whether nonlinearities are necessary to describe the relationship. If this analysis reveals an essential nonlinearity in the data, we reproduce the nonlinear relationship with a parametric regression that closely tracks the loess fit. Otherwise, we summarize the relationship in a linear fashion.

For our linear estimation, we employ ordinary least squares (OLS) analysis, along with two variants of OLS that downplay the influence of outliers, a notorious weakness of standard linear regression models. The first, denoted the robust method, is based on group medians rather than means. The second, denoted the resistant method, deemphasizes both outliers and influential observations.[15] Both methods are less drastic than discarding extreme observations. We then select the estimator that best tracks the loess fit.[16]

Figure 3 summarizes the relation between BVAP and

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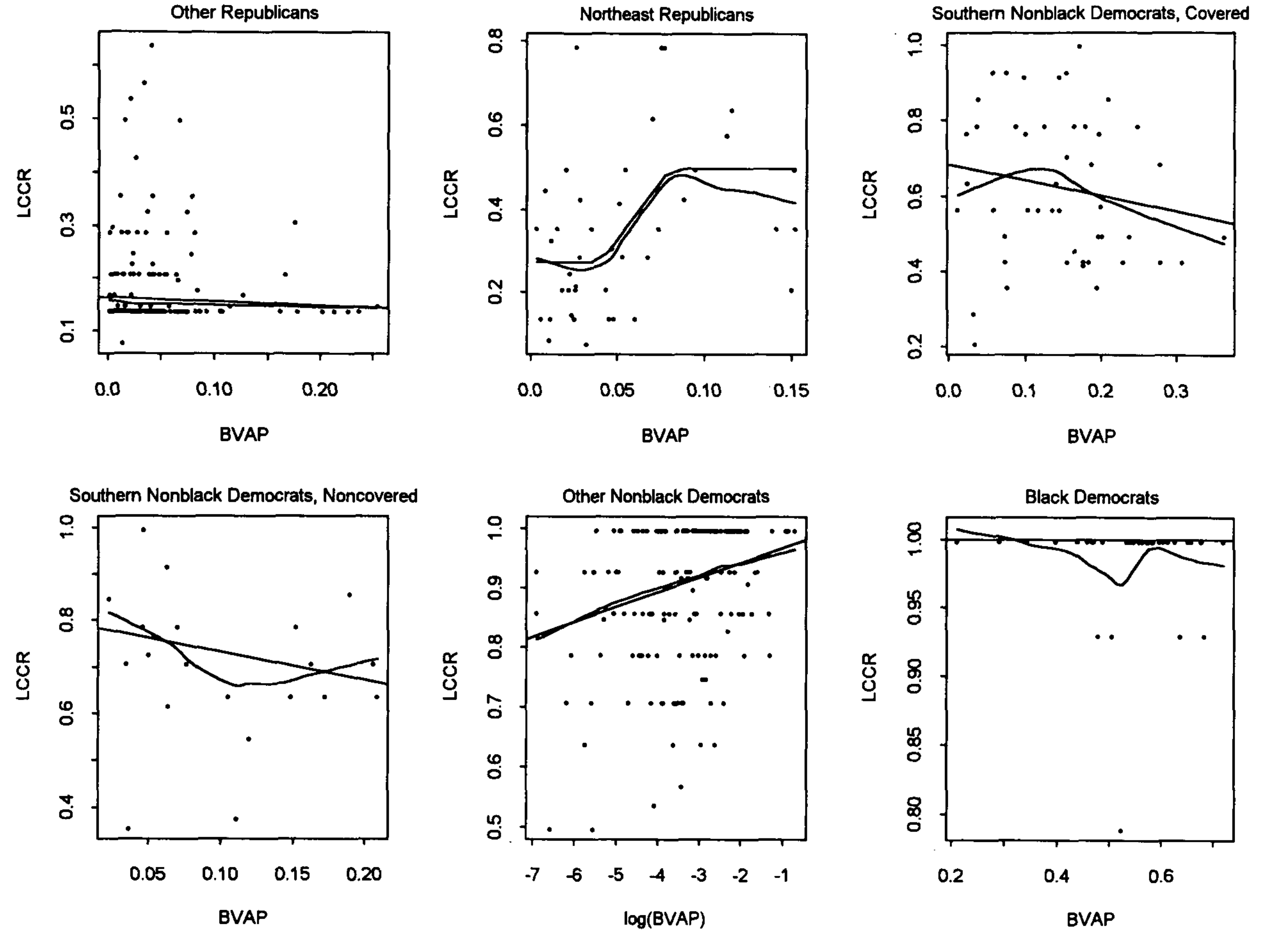
[14] The reasoning here is similar to that used in the related area of determining whether voting in a given area is racially polarized. Here, too, the courts have decided that bivariate correlations are more appropriate than mutivariate regressions. For a discussion, see Grofman, Handley, and Niemi (1992, 83–93).

[15] For the robust method we employ the rlm function in S-Plus, provided by Venables and Ripley (1994), which uses Huber's M-estimator. For the resistant method, we use the ltsreg procedure in S-Plus, which implements Rousseeuw and Leroy's (1987) least trimmed squares regression. This procedure minimizes the sum of the smallest half of the squared residuals. Venables and Ripley (1994), in chapter 8, provide details on both procedures; the ltsreg procedure produces no standard errors.

[16] As a diagnostic check, we identified influential observations via their Cook's distances and reran the linear regressions deleting the influen-

FIGURE 3. Representation Equations by Subgroup



LCCR in each subgroup. Shown in each panel of the figure are loess fits along with the fit from our best parametric estimate of the relationship. In all cases, the selected model tracks the loess fit rather well.

Table 4 provides more detail. The row labeled "Pr(*F*)" provides the probability value for an *F*-test of the models $LCCR = a + b * BVAP$ and $LCCR = a + b * s(BVAP)$, where $s(\cdot)$ denotes a nonparametric smoothing spline.[17] Only for Northeast Republicans is the nonlinearity statistically significant, as shown by the loess fit in Figure 3. For nonblack Democrats outside the South, the distribution of black voting-age population is highly skewed toward rural regions; re-expression of the independent variable as log(BVAP) corrects this skew so that a linear model may be appropriately applied.[18] The values in the row labeled *Average Estimated LCCR* calculate the value of the selected model at the mean BVAP level in the group. The resulting estimated LCCR score can then be compared to the actual median and mean values shown in the first two rows of the table. In all cases the selected model produces estimated LCCR scores close to the actual mean and median values.

The nonlinear parametric model selected for Northeastern Republicans is a step function with a ramp between the steps. The function is:

$$LCCR = \begin{cases} 0.2732 \text{ if } BVAP \le 0.04 \\ 0.2732 + 5.604 * (BVAP - 0.04) \text{ if} \\ \quad 0.04 \le BVAP \le 0.08 \\ 0.4974 \text{ if } BVAP > 0.08. \end{cases}$$

Two parameters are estimated, the first being the lower intercept and the second a term that jointly determines the slope of the ramp and the distance between the two steps. The *t*-value for first parameter is 8.2, while that of the second is 3.6.[19] As seen in Figure 3,

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tial observations. In no case was there, for example, a sign reversal, which would indicate a major problem with outliers.

[17] We use the s ( ) function in S-Plus, which fits a cubic smoothing spline with df = 4. All the loess fits in Figure 3 were generated by the S-Plus function scatter.smooth, which employs a span of ⅔. See Chambers and Hastie (1993, Section 7.4.1) for details.

[18] In category 5, all results shown are for the independent variable *log(BVAP)*. So the *F*-test indicates that, following reexpression, no additional nonlinearity is essential in describing the data.

[19] The actual function estimated, using the S-Plus code, is:

$$LCCR = [0.2732 + 5.6044x] \cdot [I(I((x \ge 0.04) * (x \le 0.08) * (x - 0.04)) + I(0.04 * (x > 0.08)))].$$

TABLE 4. Representation Equations for LCCR Scores, by Subgroups

	Subgroup					
	Nonnortheastern Republicans	Northeastern Republicans	Nonblack Southern Democrats, covered	Nonblack Southern Democrats, noncovered	Nonblack Democrats Outside South	Black Democrats
LCCR						
median	.14	.35	.57	.71	.93	1.00
mean	.19	.35	.63	.71	.89	.99
BVAP						
median	.04	.04	.15	.08	.04	.56
mean	.05	.05	.15	.10	.06	.54
Linear Model						
intercept	.20 (.01)	.27 (.04)	.67 (.06)	.74 (.07)	1.00 (.03)	.99 (.03)
slope	−.17 (.15)	1.57 (.66)	−.32 (.36)	−.32 (.58)	.03 (.01)	−.01 (.06)
Robust Model						
intercept	.17 (.006)	.25 (.04)	.69 (.06)	.77 (.05)	.99 (.02)	1.00 (0.0)
slope	−.10 (.08)	1.60 (.61)	−.41 (.36)	−.47 (.45)	.02 (.008)	.00 (0.0)
Resistant Model						
intercept	.14	.10	.57	.77	1.02	1.00
slope	0.00	4.20	−.42	−.59	.01	.00
General additive						
Pr(*F*)	.67	.02	.51	.19	.53	.37
Final Model	robust	nonlinear	robust	robust	robust	robust
Averge Est. *LCCR*	.16	.33	.62	.73	.95	1.0
N	134	42	47	21	154	37

Note: The nonlinear parametric model selected for Northeastern Republicans is discussed in the text. Average estimated *LCCR* is calculated using the final model at the mean *BVAP* level. Standard errors are given in parentheses.

the representation pattern here exhibits a clear threshold effect.

The analysis provides some interesting evidence concerning the polarization hypothesis. The estimated models in subgroups 1, 3, and 4 indicate that representatives' LCCR scores *decrease* as the percentage of black voting age population increases. Yet, the effect is sufficiently small in magnitude to prevent rejecting the hypothesis that no polarization effect exists, given the precision of the estimates of the slope coefficients. Comparisons across groups, as shown in Figure 3, clearly indicate that the polarization effect is dwarfed in magnitude by shifts across subgroups, for example, from Republican, to nonblack Democrat, to black Democrat. Therefore, the large polarization effect visible in Figure 2 seems to be an artifact of mixing together several distinct populations, none of which individually displays a similar pattern.

Notice, too, that support for minority issues is strong among nonblack Democrats, especially outside the South. Although not quite as high as for black Democrats, the LCCR scores of these representatives are generally around 80% on the index. Even in the South, the gap between nonblack Democrats and Republicans is quite significant. This highlights again a basic trade-off in reapportionment decisions: whether to place blacks in majority-minority districts, where they can increase the probability of a black Democrat being elected, or to place them elsewhere, to boost the chances of any Democrat winning a seat rather than a Republican, who is likely to be unsupportive of minority positions.

Electoral Equations

The preceding analysis suggests that LCCR scores are relatively constant within the six subgroups but differ widely across them. The percentage of BVAP may thus have its largest effect by influencing the distribution from which a representative is drawn; that is, in determining whether the elected representative is a Republican, a nonblack Democrat, or a black Democrat. In this section we estimate equations relating the size of the black voting-age population to the type of representative elected (θ):

$$P(\theta \mid BVAP). \qquad (2)$$

The small number of black representatives and their geographic concentration make it impossible to estimate the electoral equations using a trinomial logit model disaggregated by region (estimation routines in standard computer programs fail to converge). Therefore, we conducted sequential logit analysis to determine the model specification that best fits our data while retaining regional and partisan characteristics. The results from this portion of the analysis can be summarized fairly succinctly. Both portions of the sequential logit models were estimated using smoothing splines. Neither the logit for black Democrats, estimated at the national level, nor the three separate logits for nonblack representatives displayed important nonlinearities, using the nonparametric Chi-squared test.[20] Accordingly, all models were fit using a general linear model that includes a

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[20] See Chambers and Hastie (1993, 262, 306).

TABLE 5. Final Electoral Equation, Multinomial Logit

	Category	
Variables	Nonblack Democrats	Republicans
Constant	12.15 (4.17)	12.44 (4.18)
BVAP	−43.43 (14.97)	−54.01 (15.17)
BVAP*East	17.38 (7.31)	21.98 (7.93)
South	5.21 (2.91)	5.86 (2.92)
Log-likelihood	−268.34	
χ^2 (6)	262.85	
Percentage correctly predicted	61%	
Percentage Improvement	20%	

	Predicted			
Actual	Black Democrats	Nonblack Democrats	Republicans	Total
Black Democrats	35	2	0	37
Nonblack Democrats	2	147	73	222
Republicans	0	92	84	176
Total	37	241	157	435

Note: Standard errors are given in parentheses.

dummy variable intercept term for the South and an interactive slope term *East*BVAP*.

The resulting multinomial logit model, with various summary statistics, is shown in Table 5. Like all multinomial logit models, it is appropriate only if the data display the independence of irrelevant alternatives (IIA) property (Greene 1993, 670–2). A procedure devised by Hausman and McFadden (1984) is commonly employed to test for the validity of the IIA assumption; application of this test to the model in Table 5 fails to reject the null hypothesis that the property holds.

Figure 4 displays the results of the electoral analysis in an easily interpretable fashion. Each panel shows the estimated probabilities that a representative is a Republican, nonblack Democrat, or black Democrat for all possible levels of BVAP in a given region. At any level of BVAP, the three probabilities sum to one; the most likely representative at any level of BVAP is given by the uppermost of the three lines. The figures show the dramatic effect of black voting-age population on the partisan and racial identity of representatives. If a district has low levels of BVAP (0–10% in the South, 0–5% outside the South), the most likely partisan identity of the representative from that district is Republican, and it is exceedingly unlikely the representative is a black Democrat. At somewhat higher levels of BVAP (10–30% in the South, 5–30% in the Northwest, 5–20% in the Northeast), the most likely partisan identity of a representative is Democrat, though it remains possible the representative is Republican; in this range it remains quite unlikely the representative is black. At a sufficient concentration of minority voters (starting at 30% in the South and Northwest, at 20% in the Northeast), the probability that the representative is a black Democrat increases very rapidly, soon becoming a virtual certainty.

There are important differences across regions, perhaps best summarized by the concentration level of BVAP required to achieve a 50% probability that the elected representative is a black Democrat, that is, the point of equal opportunity for minority voters to elect their candidate of choice. As shown in Figure 4, in the South the required level of BVAP is 40.3%; in the Northwest, 47.3%; in the Northeast, only 28.3%. These figures carry a number of important implications. It is rarely necessary for minority voters to be a clear majority within a district to have a good chance of electing a minority representative, and the 65% rule enforced by the courts certainly seems excessive. By the same token, black candidates seem to have a fair chance of winning election, even in districts with a white majority (so-called minority-minority districts). Thus, Swain's emphasis on the electoral possibilities in these districts is consistent with our findings.

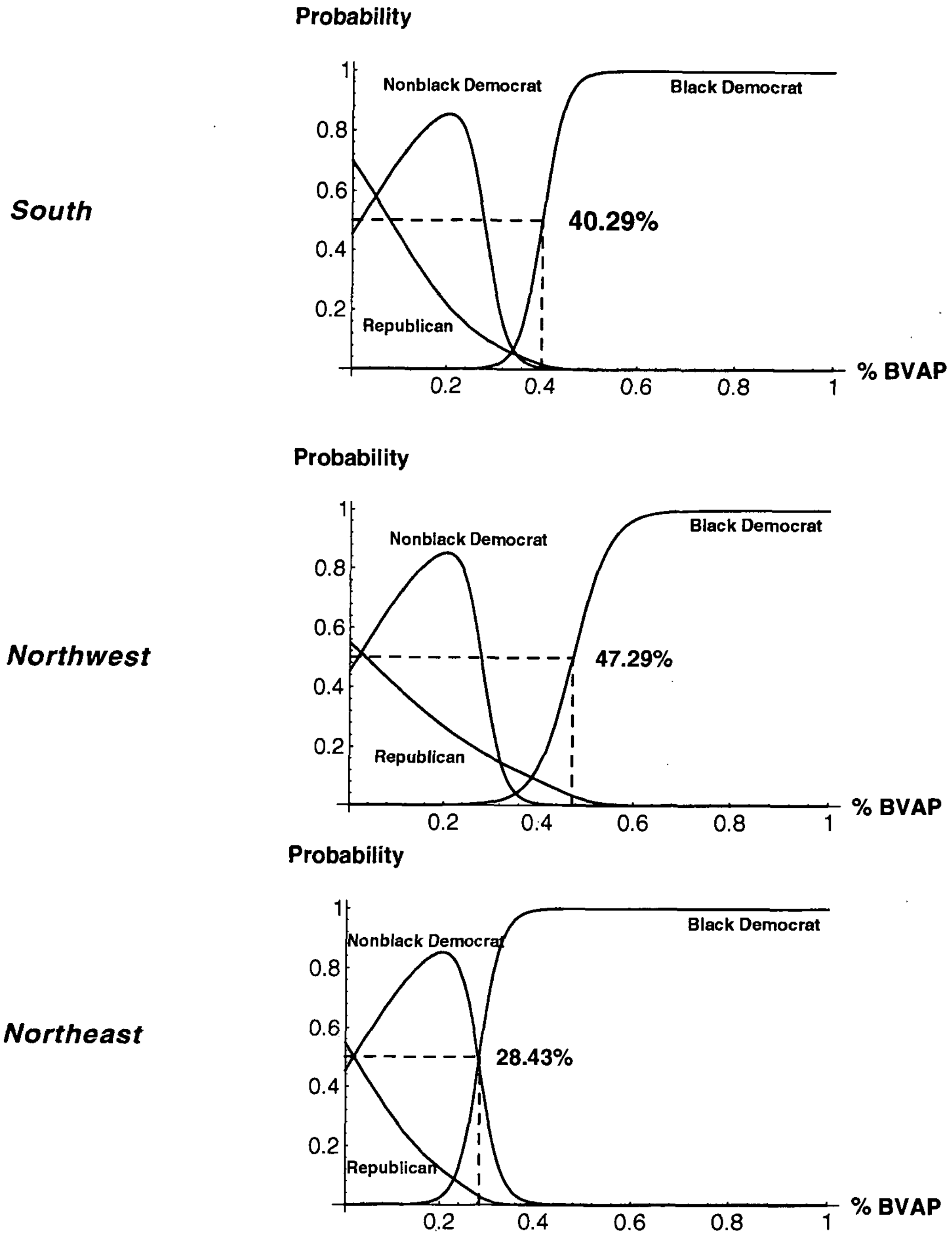
Figure 4 suggests that the probability of electing a Republican in the South is fairly high at low levels of black participation. The likelihood is far less in the Northwest, while the Northeast presents a middle case. Furthermore, nonblack Democrats retain a fairly high chance of winning in the Northeast, even with significant portions of black voters in the district. This may be due to the fact that liberal northeastern Democrats do a good job of representing their black constituents' concerns. It also may reflect the presence of liberal Democratic machines in many large northeastern cities.

Finally, we point out the fact that the electoral equations estimate the probability of different types of representatives being elected, *given present turnout*. If minority turnout were to increase so that minority candidates would stand a greater chance of winning in any given region, for example, then this would be reflected in Figure 4 by raising the estimated black Democrat probability line. Thus, although turnout effects are not explicitly built into our estimation procedure, they do influence our final results.

OPTIMAL DISTRICTING FOR SUBSTANTIVE MINORITY REPRESENTATION

The previous section estimated representation effects, the influence of minority constituents on their legislator's voting behavior, and electoral effects, the relation between the composition of the electorate and the type of representative elected. Our analysis showed that the representation effect was small or negligible: Within any given subgroup of representatives, an increase in the percentage of black voters in a district had little influence on voting patterns. Taken alone, this information might seem to lend support for majority-minority dis-

FIGURE 4. Probability of Election of Three Types of Representatives by Percentage Black in District, by Region



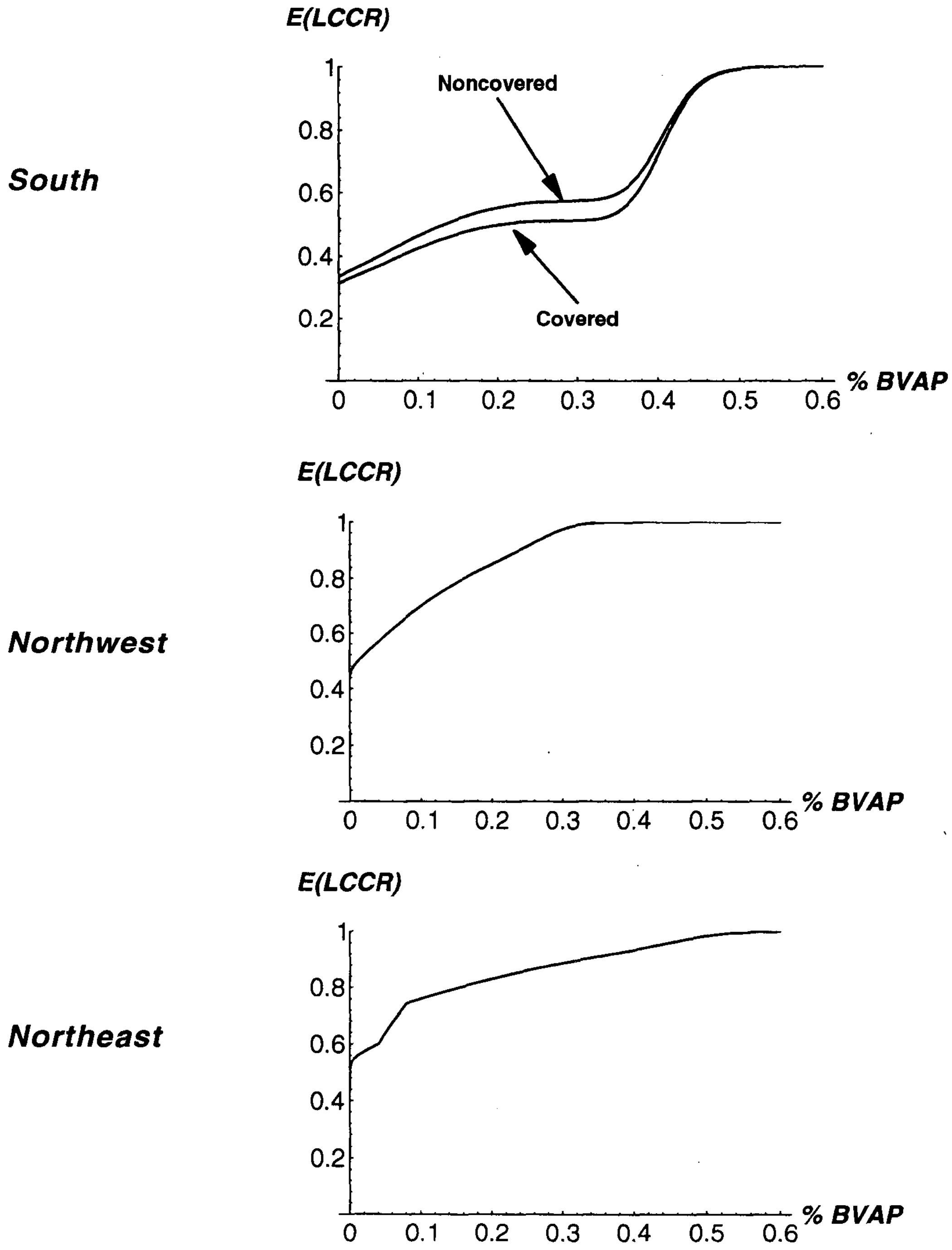
tricts, as black representatives do, indeed, consistently show the highest level of support for minority issues.

Yet, the electoral effect of minority voters proved to be much more significant, as the BVAP in a district has a substantial influence on the partisan and racial characteristics of the representative. And while it is certainly true that majority-minority districts are quite likely to elect a minority representative, Figure 4 indicates that high concentrations may be unnecessary to secure minority electoral opportunities. The basic question, then, is whether the marginal gain in the probability of electing a black Democrat from a majority-minority district is sufficient to offset the marginal loss of influence in other districts.

Expected LCCR Scores

To answer this question, we first calculate the expected LCCR score of a representative in each geographic region, depending on the level of BVAP. This is done by simply combining figures 3 and 4. For each region, we estimate the probability of electing each type of repre-

FIGURE 5. Expected Score on Leadership Conference on Civil Rights Voting Index by Percentage Black in District, by Region



sentative, multiplied by the expected representation score of that type. That is, for a given region and level of BVAP:

$$E(LCCR \mid BVAP) = \sum_{\theta} E(LCCR \mid BVAP, \theta) * P(\theta \mid BVAP), \quad (3)$$

which is just the combination of equations 1 and 2. Figure 5 illustrates the result of these calculations.[21]

In southern districts, both covered and noncovered, the relationship is positive up to about 25% BVAP, then relatively flat to 35% BVAP, and then rises steeply up to

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[21] Estimated standard errors for these calculations are shown in Appendix B.

50% BVAP. The noncovered line follows a similar pattern but is slightly higher than for covered districts. Overall, these findings attest to the importance of white Democrats in southern districts. Although neither Republicans nor Democrats represent black interests as well as do black Democrats, white Democrats are still more supportive than are Republicans. So, although adding black voters to a district does not change the voting pattern of any given representative (if anything, it slightly *decreases* black representation), it does increase the probability of electing a Democrat over a Republican. Once again, the electoral effect dominates the representation effect.

This pattern is even clearer in the other two geographic regions. In the Northwest, the relationship increases steadily to about 35% BVAP, when it levels off at 1.00; by the time a district reaches 35% BVAP, it is virtually certain to elect a Democrat. As any Democrat in this area represents black interests well, the marginal effect of adding minority constituents beyond 35% is negligible.

In the Northeast, the relationship rises sharply until about 10% BVAP, due to the threshold effect from Northeastern Republicans, and then rises more gently to about 55%. Notice that with only 10% BVAP in a district, the expected support score is nearly 80%. Again, this reflects the general liberal outlook of both Democrats and Republicans from the Northeast.

Simulation Equations: The Overall Representation of Black Interests

Given the relationships in Figure 5, what districting strategy maximizes substantive minority representation in each region of the country? That is, given a fixed number of districts and percentage of black voters in the population, which configuration maximizes the average support for minority legislation in a state's congressional delegation? Notice that by posing the question in this manner, we automatically account for the fact that placing more black voters in one district will necessarily reduce their number in surrounding districts.

An argument could be made at this point that the relevant statistic for measuring representation is the median LCCR score of a state's congressional contingent, rather than the average score. Three factors indicate that calculating average scores is preferable, however. First, maximizing state-by-state medians will not necessarily maximize the national median. Second, while median positions are relevant if the policy at issue is unidimensional, they have little meaning for multidimensional policies. For instance, the distribution of pork-barrel benefits to minority constituents, one of the purported benefits of majority-minority districts, is an inherently multidimensional problem. Third, if voting scores are taken to be probabilities of voting in favor of minority-supported issues, then, again, maximizing the average score is appropriate.[22]

We analyze the question of optimal districting as a constrained nonlinear maximization problem. Consider a state with n districts and P_b percentage of black voters. Let $\mathbf{b} = (b_1, b_2, \ldots, b_n)$ represent the vector of the percentage of black voters in each district. Let $LCCR(b_i)$ be the expected LCCR score in district i if it contains b_i percentage of black voters, as calculated from Figure 5. Then, the problem to be solved is:

$$\max_{\mathbf{b}} \sum_{i=1}^{n} LCCR(b_i), \qquad \textbf{(4)}$$

subject to:

$$\text{(i) } b_i \geq 0,$$

$$\text{(ii) } b_i \leq 1,$$

$$\text{(iii) } b_i \leq n * P_b,$$

$$\text{(iv) } \sum_{i=1}^{n} b_i = n * P_b.$$

Notice that the total b_i's must be equal to $n * P_b$. For instance, in a state with three districts and 15% black voters, a single district could have 45% black voters if the other two have none. In general, the total of the BVAPs in all districts will equal the average BVAP in the state times the number of districts. Constraints (i) through (iii) state that the percentage of black voters in a district must be nonnegative, that it cannot be greater than the maximum population in any one district, and that it cannot be greater than the total number of minority residents in the state. Constraint (iv) indicates that all minority residents must be members of some district.

For purposes of comparison, we also calculated the distribution of minority voters that would maximize the expected number of minority candidates elected to office, that is, descriptive rather than substantive representation. Here, the maximization problem was the same as equation 4, except that the objective function was to $\max_{\mathbf{b}} \Sigma_{i=1}^{n} P_{BD}(b_i)$, where $P_{BD}(\cdot)$ is the probability of electing a black Democrat, calculated from the logit analysis in Table 5.

Results and Discussion

Simulation results for states with five districts are illustrated in Table 6.[23] As shown, outside the South, black representation is maximized by distributing black voters equally across districts. The reasoning behind this comes from Figure 4: The marginal gain in the representation effect from placing more minority voters in any given district is less than the marginal loss from the electoral effect elsewhere. In other words, the most important objective for minority representation in these areas is to elect Democrats, either black or nonblack. Majority-minority districts make little sense in this context, unless

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[22] See Aranson, Hinich, and Ordeshook (1974) and Hinich (1977) for discussions of the correct objective function in similar voting games, arguing in favor of means over medians.

[23] The simulations were performed using Microsoft Excel's Solver function, which uses the Newton-Raphson hill-climbing algorithm. For an overview of this procedure, see Press, Flannery, Teukolsky, and Vetterling (1986, 254–9).

TABLE 6. Optimal Divisions of Minority Voters

Total BVAP in State	Optimal Percentage Divisions to Maximize LCCR Score: Northwest	Northeast	South	Black Representatives, South
3%	(3, 3, 3, 3, 3)	(3, 3, 3, 3, 3)	(3, 3, 3, 3, 3)	(0, 0, 0, 0, 15)
5	(5, 5, 5, 5, 5)	(5, 5, 5, 5, 5)	(5, 5, 5, 5, 5)	(0, 0, 0, 0, 25)
8	(8, 8, 8, 8, 8)	(8, 8, 8, 8, 8)	(8, 8, 8, 8, 8)	(0, 0, 0, 0, 40)
10	(10, 10, 10, 10, 10)	(10, 10, 10, 10, 10)	(1, 1, 1, 1, 46)	(0, 0, 0, 0, 50)
15	(15, 15, 15, 15, 15)	(15, 15, 15, 15, 15)	(7, 7, 7, 7, 47)	(0, 0, 0, 0, 75)
18	(18, 18, 18, 18, 18)	(18, 18, 18, 18, 18)	(0, 0, 0, 45, 45)	(0, 0, 0, 45, 45)
20	(20, 20, 20, 20, 20)	(20, 20, 20, 20, 20)	(2, 2, 2, 47, 47)	(0, 0, 0, 50, 50)
25	(20, 20, 25, 30, 30)	(25, 25, 25, 25, 25)	(10, 10, 11, 47, 47)	(0, 0, 0, 62.5, 62.5)
30	(30, 30, 30, 30, 30)	(30, 30, 30, 30, 30)	(4, 5, 47, 47, 47)	(0, 0, 50, 50, 50)

Note: Entries in the table are the percentage of black voting-age population in each district.

they confer significant nonpolicy benefits, as they create greater possibilities for electing Republicans in other districts.

In the South, however, the picture is slightly different. Recall that Figure 5 showed a flat region of expected LCCR scores from about 25–35% BVAP. Given this fact, there is no reason to construct districts with a BVAP in this range, as those voters could be usefully allocated to other districts. Furthermore, significant improvements in representation occur in the 35–50% range. Combining these effects, the formula to maximize black representation in the South is to construct as many districts as possible that are approximately *47% black*, with the remaining black voters distributed across other districts as evenly as possible.[24]

In fact, this result is not too different from the current districting rule of thumb in the South, which holds that as many concentrated black districts should be constructed as possible. Yet, "concentrated" in our analysis does not include actual majority-minority districts, which according to our estimation are unnecessary to maximize black representation. Rather, districts that are a bit less than majority-minority give black representatives a substantial chance of winning office (though not assuredly so), while supporting minority influence in other districts.

To make this point more clearly, the rightmost column in Table 6 shows the allocation of black voters to districts in the South that would maximize the expected number of minority candidates elected to office. In nearly all cases, these solutions vary from an allocation that would maximize LCCR scores. And whenever a discrepancy between the figures that would maximize each type of representation (i.e., the last two columns of Table 6) appears, the pattern is to concentrate minority voters more heavily than necessary to maximize representation, even to the point of constructing districts with 75% black voters.

We conclude that a trade-off between descriptive and substantive representation does exist: Districting schemes that maximize the number of minority representatives do not necessarily maximize substantive minority representation. In practice, policymakers must choose their objective function, be it to maximize black representation, maximize the number of black representatives, or some combination of these. Depending on the objective chosen, a particular districting strategy will be optimal, and it can be calculated by substituting the appropriate weights into equation 4.

How important are the effects discussed in this paper? If, in the end, rearranging districts to maximize black representation would lift expected LCCR scores only a point or two, then the mathematical complexities of the preceding discussion would not amount to much. To determine the magnitude of the effects, we simulated the maximum and minimum expected LCCR by state. In effect, we calculated the optimal gerrymander and antigerrymander for each state, given the state's region, number of electoral districts, coverage under Section 5 of the Voting Rights Act, and percentage of minority voting-age population.

The results of this estimation are shown in Table 7. The second and third columns show the number of districts and BVAP in each state. The next two columns show the minimum and maximum possible expected LCCR scores, respectively. The last column, which gives the difference between these two figures, provides a measure of the state's susceptibility to gerrymandering. Not surprisingly, the drawing of district boundaries in states with few districts or few minority voters would have little effect on the representation scores of the state's congressional delegation. Gerrymanders would make an appreciable difference, however, in states with a high percentage of black voters, especially in the South. In Louisiana the difference is 21.3 points, in Mississippi, 23.3 points, in South Carolina, 19.9 points, and so on. The overall national effect on LCCR scores, weighting the swing in each state by its seats in the U.S. House, would be 13.1. We therefore conclude that the representation effects estimated above are not only statistically significant but also substantively important in a number of states.

These simulation results are based on estimates of electoral and representation effects from the 103rd Congress. To estimate the sensitivity of the results to changing political conditions, as well as differing param-

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[24] These results are somewhat similar to those found in Grofman, Griffin, and Glazer (1992), who also estimate the effect of changing BVAP district by district on overall congressional liberalism, as measured by ADA scores.

TABLE 7. Optimal Gerrymander by State

State	Districts	BVAP	Minimum E(LCCR)	Maximum E(LCCR)	Difference
AK	1	3.82%	56.6	56.6	0.0
AL	7	25.30	45.7	62.5	16.7
AR	4	13.70	45.4	52.2	6.7
AZ	6	2.72	50.7	53.9	3.2
CA	52	6.98	49.0	63.7	14.7
CO	6	3.69	51.9	56.3	4.4
CT	6	7.46	57.2	72.7	15.5
DE	1	15.25	79.9	79.9	0.0
FL	23	11.39	40.0	48.1	8.1
GA	11	24.56	46.7	64.9	18.2
HA	2	2.21	51.4	52.7	1.3
ID	2	0.34	46.6	47.1	0.5
IL	20	13.42	60.9	75.9	15.1
IN	10	7.02	50.3	63.7	13.5
IO	5	1.47	48.7	50.7	2.0
KS	4	5.19	55.1	59.7	4.6
KY	6	6.57	38.9	42.1	3.2
LA	7	27.84	48.8	70.1	21.3
MA	10	4.41	54.0	64.4	10.4
MD	8	23.43	62.1	85.2	23.1
ME	2	0.39	52.3	54.0	1.7
MI	16	12.74	53.4	74.8	21.4
MN	8	1.82	48.9	51.7	2.8
MO	9	9.71	50.9	69.2	18.4
MS	5	31.62	51.4	74.7	23.3
MT	1	0.23	46.5	46.5	0.0
NC	12	18.01	44.7	56.6	11.9
ND	1	0.46	47.6	47.6	0.0
NE	3	3.15	52.7	55.0	2.2
NH	2	0.57	52.6	54.5	1.9
NJ	13	12.34	57.2	77.8	20.6
NM	3	1.83	50.0	51.7	1.7
NV	2	5.77	58.7	61.0	2.3
NY	31	14.74	57.6	79.6	22.0
OH	19	9.75	50.6	69.3	18.8
OK	6	6.64	54.0	62.9	9.0
OR	5	1.39	48.5	50.5	2.0
PA	21	8.45	54.2	74.8	20.6
RI	2	3.25	59.1	61.0	2.0
SC	6	26.88	48.2	68.1	19.9
SD	1	0.38	47.2	47.2	0.0
TN	9	14.35	43.6	52.4	8.8
TX	30	11.20	38.4	47.0	8.6
UT	3	0.66	47.2	48.3	1.1
VA	11	17.56	42.4	55.9	13.4
VT	1	0.32	53.7	53.7	0.0
WA	9	2.71	49.8	53.9	4.1
WI	9	4.04	50.9	57.1	6.2
WV	3	2.92	57.5	60.8	3.3
WY	1	0.69	48.4	48.4	0.0

eter estimates, we altered each of the estimated coefficients in the electoral and representation equations by one standard deviation. The coefficients with the greatest effect on expected LCCR scores are the constants in the electoral equation (see Table 5). These coefficients provide the base probability of voting for either a nonblack Democrat or a Republican. Hence, changes in these parameters have an important substantive interpretation: An alteration in their magnitude corresponds to either a national tide favoring Democrats (if the change is positive) or Republicans (if the change is negative).

We reestimated the districting schemes in covered southern districts that would maximize minority representation if either of these constants changed by as much as one standard deviation. Interestingly, under a Democratic surge scenario, to maximize black representation, minority districts should be *more concentrated* than before, up to about *55% BVAP*. Under a Republican surge scenario, the optimum would be *less concentrated* districts, on the order of 40% BVAP. These simulation results have a number of important implications. First, our recipe for concentrated minority districts in the South is robust to changes in underlying political currents. Second, minorities are best insulated against a Republican tide by less concentration rather than more. This somewhat counterintuitive result is driven by the expected shift in support from nonblack Democrats to black Democrats in moderately concentrated districts.

Similar analyses could be conducted of other scenarios, for example, increases in black voter turnout (by changing the coefficient on BVAP in the electoral equations), convergence of southern voting patterns with national ones (by changing the coefficient on the *South* dummy variable), or increased responsiveness of Democrats or Republicans to minority issues (by changing the coefficient on BVAP in the representation equations).

CONCLUSION

The appropriate representation of minorities was central to the debates at the founding of the American Republic. The *Federalist* 10, for example, warned against the dangers of majority tyranny and suggested ways to design institutions to offset this possibility. The current controversy over minority-controlled voting districts is thus but another conversation in the long-running debate of democratic theory.

This paper exploits the link between the composition of the electorate and legislators' roll-call voting behavior to provide statistical evidence on the effects of racially oriented gerrymandering. First, we examined the relation between the percentage of minority voters in a district and the representative's support for minority issues, which we labeled the representation effect. Then, we calculated the electoral effect, that is, the influence of differing levels of black voters in a district on the racial and partisan characteristics of the representative. Next, we combined the two effects to calculate the distribution of minority voters across districts that maximizes their substantive representation in Congress.

Our principal conclusions are these. Outside the South, substantive minority representation is best served by distributing black voters equally among all districts. In the South, the key is to maximize the number of districts with slightly less than a majority of black voters. In passing, we note that black candidates have a healthy chance of winning election outside majority-black districts and that the 65% rule enforced by the courts is almost certainly too stringent; it dilutes rather than increases overall minority voting strength. Overall, maximizing the number of minority representatives does not necessarily maximize minority representation, as measured by roll-call voting behavior.

Relating our findings to larger themes in U.S. politics, we note that the debate over majority-minority districts has three prongs: as a constitutional matter, as politics, and as public policy. According to legal scholars, the constitutional status of minority districting has evolved through two distinct phases. The first began with the signing of the Voting Rights Act in 1965 and concentrated on the harm of vote dilution, to which concentrated minority voting districts were the favored solution. The second began in 1993 with the *Shaw* v. *Reno* decision and continued through the recent *Bush* v. *Vera* and *Shaw* v. *Hunt* cases. It focuses on the harm of separating voters by race, ruling that a districting plan driven by predominantly racial considerations must undergo strict scrutiny to prove its constitutionality. Our findings imply that these two views need not be irreconcilable, since majority-minority districts may, in fact, dilute the overall influence of minorities on public policy. If this is the case, then bizarrely shaped districts like the North Carolina Twelfth are, indeed, an unnecessary feature of the political landscape.

From a political point of view, the rise in the number of majority-minority districts has occurred as the result of a sometimes tacit, sometimes overt collusion between representatives of the minority community and Republicans. In many southern state legislatures, these two groups formed voting blocs when passing redistricting plans, and the Justice Department under Republican presidents was eager to create the maximum possible number of majority-minority districts. It is not obvious that this arrangement works to the detriment of minority voters—indeed, in some instances it may increase their overall influence—but our results show that generally more votes are lost than are gained by this strategy. The politics of districting may well be changing, however; interparty electoral coalitions may have had more allure when it appeared that Democrats had a permanent majority in the U.S. House.

Finally, regarding the policy aspects of the issue, our results show that a trade-off does exist between substantive and descriptive representation, and this poses a difficult decision. On the one hand, some would argue that there is an independent value to having minority representatives in office, making Congress look more like the population it represents. On the other hand, past a certain point, an increase in the number of minority representatives comes at the cost of votes in favor of minority-sponsored legislation. It is disconcerting that no minorities were elected to Congress from the South between 1898 and 1972. But it is hard to argue that minority voters in Georgia are better served overall when their congressional delegation goes from nine Democrats and one Republican to three black Democrats and eight white Republicans in the span of two years. Thus, there is a trade-off to be made, striking a balance between electing minority representatives to office and enacting legislation favored by the minority community.

APPENDIX A. BILLS INCLUDED IN VOTING SCORES

Leadership Conference on Civil Rights (LCCR) Votes

No.	Bill No.	Bill Title and Description	LCCR Position
LCCR1	HR 6	Elementary and Secondary Education Reauthorization—Republican substitute	Oppose
LCCR2	HR 6	Elementary and Secondary Education Reauthorization—Conference report	Support
LCCR3	HR 1	Family and Medical Leave—Optional benefits	Oppose
LCCR4	HR 1	Family and Medical Leave—Passage	Support
LCCR5	HR 2	National Motor-Voter Registration—Proof of citizenship	Oppose
LCCR6	HR 2	National Motor-Voter Registration—Conference report	Support
LCCR7	HR 4092	Omnibus Crime Bill—Republican "Racial Justice" Act	Oppose
LCCR8	HR 1133	Violence against Women—Passage	Support
LCCR9	HR 51	D.C. Statehood—Passage	Support
LCCR10	H J Res 103	Balanced Budget Constitutional Amendment—Passage	Oppose
LCCR11	HR 5	Striker Replacement—Passage	Support
LCCR12	HR 4822	Congressional Compliance—Passage	Support
LCCR13	HR 1025	Brady Bill—Conference report	Support
LCCR14	HR 2401	Fiscal 1994 Defense Authorization—Amendment to weaken gay ban	Support

Congressional Quarterly Key Votes

No.	Bill No.	Bill Title and Description	Liberal Position
CQ1	HR 1	Family and Medical Leave—Passage	Support
CQ2	H Con Res 64	Fiscal 1994 Budget Resolution—Adoption	Support
CQ3	HR 1335	Fiscal 1993 Supplemental Appropriations—Rule	Support
CQ4	HR 2295	Fiscal 1994 Foreign Operations Appropriations—Aid to Russia	Oppose
CQ5	HR 2518	Fiscal 1994 Labor, HHS, Education Appropriations—Abortion prohibition	Oppose
CQ6	HR 2264	1993 Budget Reconciliation—Adoption	Support

APPENDIX A. Continued

Congressional Quarterly Key Votes continued

No.	Bill No.	Bill Title and Description	Liberal Position
CQ7	HR 1340	Resolution Trust Company—Passage	Support
CQ8	HR 2401	Fiscal 1994 Defense Authorization—Amendment to weaken gay ban	Support
CQ9	HR 1845	National Biological Survey—Nonfederal property	Support
CQ10	HR 2445	Fiscal 1994 Energy and Water Appropriations—Previous question	Support
CQ11	H Con Res 170	Somalia Troop Removal—March 31 deadline	Support
CQ12	HR 1025	Brady Bill—Passage	Support
CQ13	HR 3450	NAFTA Implementation—Passage	Oppose
CQ14	HR 322	Mining Law Overhaul—Passage	Support
CQ15	HR 3	Campaign Finance—Rule (HRes. 319)	Support
CQ16	HR 3400	Fiscal 1994 Spending Cuts and Govt. Restructuring—Penny-Kasich Amendment	Oppose
CQ17	HR 3425	Department of Environmental Protection—Rule	Support
CQ18	H Con Res 218	Fiscal 1995 Budget Resolution—Defense Cuts	Support
CQ19	H J Res 103	Balanced Budget Constitutional Amendment—Passage	Oppose
CQ20	H Con Res 218	Fiscal 1995 Budget Resolution—Instruct Conferees	Oppose
CQ21	HR 4296	Assault Weapons Ban—Passage	Support
CQ22	S 636	Abortion Clinic Access—Conference Report	Support
CQ23	HR 4301	Fiscal 1995 Defense Authorization—Bosnia Arms Embargo Unilateral Termination	Oppose
CQ24	HR 3626	Revising Restrictions on the Regional Bell Companies—Passage	Support
CQ25	HR 4624	Fiscal 1995 VA, HUD Appropriations—Space Station	Oppose
CQ26	HR 518	California Desert Protection—Hunting Exception	Oppose
CQ27	HR 4604	Entitlement Spending Control—Stenholm Substitute	Oppose
CQ28	HR 3355	Omnibus Crime Bill—Rule	Support
CQ29	HR 3355	Omnibus Crime Bill—Conference Report	Support
CQ30	S 349	Lobbying Disclosure—Rule	Support
CQ31	H J Res 416	U.S. Troops in Haiti—Immediate Withdrawal	Oppose
CQ32	HR 5110	General Agreement on Tariffs and Trade—Passage	Oppose

APPENDIX B: ESTIMATION OF STANDARD ERRORS

Since the equation for $E(LCCR)$ involves the product of two random variables, one must calculate standard errors using what is known as the δ-method. Let the probability of electing a representative of type θ from region r in a district with black voting-age population $BVAP$ be $p(\theta \mid BVAP, r)$, where $\theta \in \{R, BD, NBD\}$. Let the expected LCCR score of that representative be $\mu(LCCR \mid \theta, BVAP, r)$. For convenience, we will abbreviate these quantities as $p(\theta)$ and $\mu(\theta)$. Then, we wish to estimate, for a given region and value of BVAP,

$$\mathrm{Var}\left(\sum_{\theta} p(\theta)\mu(\theta)\right). \tag{B-1}$$

This expression involves the product of two subexpressions, each of which must be treated as a random variable. To estimate the overall standard error, we use the δ method, which is described in Rice (1988, 143). Essentially, the δ method involves rewriting the function to be estimated, so that a product is converted into a series of sums. In our case, we use the fact that

$$\hat{p}\hat{\mu} - p\mu = \hat{p}(\hat{\mu} - \mu) + \mu(\hat{p} - p)$$

to write the variance of equation B-1 as:

$$\begin{aligned} \mathrm{E}(\hat{p}\hat{\mu} - p\mu)^2 &= \sum_{\theta}\sum_{\theta'} \hat{p}(\theta)\hat{p}(\theta')\mathrm{Cov}(\hat{\mu}(\theta), \hat{\mu}(\theta')) \\ &\quad + \sum_{\theta}\sum_{\theta'} \hat{\mu}(\theta)\hat{\mu}(\theta')\mathrm{Cov}(\hat{p}(\theta), \hat{p}(\theta')) \qquad \textbf{(B-2)} \\ &= \sum_{\theta} \hat{p}(\theta)^2\,\mathrm{Var}(\hat{\mu}(\theta)) + \sum_{\theta}\sum_{\theta'} \hat{\mu}(\theta)\hat{\mu}(\theta')\mathrm{Cov}(\hat{p}(\theta), \hat{p}(\theta')), \end{aligned}$$

since the estimates of the $\hat{\mu}$ terms are taken from different subsets of the data and are therefore expected to be independent from one another.

The values of $\hat{p}(\theta)$ and $\hat{\mu}(\theta)$ come from substituting given values of θ and BVAP directly into the estimated representation and electoral equations for any given region. The variance of $\hat{\mu}$ for a given value of BVAP is given by:

$$\mathrm{Var}(\hat{\mu}(\theta)) = [1, BVAP]\begin{bmatrix} s_{ii}^2 & s_{si}^2 \\ s_{si}^2 & s_{ss}^2 \end{bmatrix}\begin{bmatrix} 1 \\ BVAP \end{bmatrix},$$

where s_{ii}^2 is the estimated variance of the intercept term in the representation equations, s_{ss}^2 is the estimated variance of the slope, and s_{si}^2 is the covariance between the two.

The covariance terms from the electoral equations are more complicated. Let β_1 be the vector of four coefficients for nonblack Democrats from Table 5, and let β_2 be the four coefficients for Republicans. Let M be the (8×8) variance-covariance matrix from the estimation of the electoral equation. Then, a Taylor series expansion indicates that

$$\mathrm{Cov}(p(\theta), p(\theta')) = \nabla_{\binom{\beta_1}{\beta_2}} p(\theta) \cdot M \cdot \nabla_{\binom{\beta_1}{\beta_2}} p(\theta').$$

The terms $\nabla_{\binom{\beta_1}{\beta_2}}$ are the gradients of the probability functions with respect to the vectors of coefficients for a given type of representative, value of BVAP, and region. These values complete the necessary components to calculate the right-hand side of equation B-2. The resulting standard errors of E(LCCR) for given values of BVAP are shown in Table B-1.*

In general, the greatest uncertainty occurred for levels of BVAP between 30% and 40%. This is not surprising, as relatively few electoral districts currently fall in that range of concentration. The standard error for lower and higher values of BVAP were fairly small, however, especially for levels of BVAP of 45% and more.

*The calculations were performed using a Mathematica program designed by the authors. Copies of the mathematica code are available from the author upon request.

TABLE B-1. Estimated Standard Errors for *E*(*LCCR*) Equation, Calculated by δ Method

BVAP	Northeast	Northwest	South	South Covered
0.0	0.045	0.037	0.060	0.054
0.05	0.057	0.042	0.051	0.048
0.10	0.098	0.055	0.045	0.042
0.15	0.135	0.062	0.049	0.044
0.20	0.160	0.057	0.060	0.049
0.25	0.173	0.0425	0.073	0.056
0.30	0.172	0.0182	0.090	0.066
0.35	0.157	0.003	0.126	0.110
0.40	0.122	0.0004	0.212	0.219
0.45	0.069	0.00005	0.085	0.085
0.50	0.026	$6.12*10^{-6}$	0.014	0.014
0.55	0.007	$7.18*10^{-7}$	0.002	0.002
0.60	0.002	$8.34*10^{-8}$	0.0003	0.0003
0.65	0.0005	$9.60*10^{-9}$	0.00004	0.00004
0.70	0.00014	$1.09*10^{-9}$	$6.34*10^{-6}$	$6.03*10^{-6}$

In the Northwest, the standard errors are uniformly modest, while in the South and Northeast they are somewhat higher for intermediate levels of BVAP. It is, therefore, highly improbable that alternative districting strategies could increase minority representation in northwestern states. In the South and Northeast, although the districting rule we derive is most compatible with the data, it is not impossible that alternative strategies might perform better.

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Voting and Vetoing

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The consequences of veto power in committees is analyzed using the approach of noncooperative bargaining theory. It is first shown that in equilibrium nonveto players do not share in the benefits gained by the decision making of the committee, that is, in every equilibrium outcome of the bargaining game, nonveto players earn zero. Some measures for reducing the excessive power of veto members in committees are analyzed. Specifically, I study the effects of imposing a deadline on negotiations and of expanding the committee by increasing the number of nonveto players. Quantitative results are given for the case of the UN Security Council.

In 1945, only a few weeks before the total victory over Nazi Germany, the founders of the United Nations met in San Francisco to continue discussions on the framework and charter of this institution. A year earlier, at Yalta, it had already been decided to establish the Security Council—a voting body separate from the General Assembly. The main objective of this council, according to the UN charter, was "primary responsibility for maintenance of peace and international security." Unlike the General Assembly, the Security Council was to consist of both permanent and nonpermanent members. One of the most important issues on the San Francisco agenda was the Yalta formula on voting in this council. In the precise words of the UN charter, the voting procedure was set as follows[1]:

1. Each member of the Security Council should have one vote.
2. Decisions of the Security Council on procedural matters should be made by an affirmative vote of seven members [out of eleven].
3. Decisions of the Security Council on all other matters should be made by an affirmative vote of seven members including the concurring vote of the permanent members....

Although the Yalta formula restricted the veto power of permanent members (the Soviet Union, the United States, Great Britain, France, and China) to issues of substance, it was agreed in San Francisco (after massive pressure by the Soviet Union) that the decision regarding whether a certain issue should be deemed a matter of procedure or substance would not be regarded as a procedural decision. In practice, therefore, the right of veto could be invoked on every issue on the agenda.

The idea of granting permanent members veto power evolved directly from the fact that the enforcement of many Security Council resolutions would require the military and financial support of the superpowers. Hence, without the unanimous consent of the permanent members, no effective implementation of Security Council resolutions could be expected. The argument was, therefore, not in favor of allowing the superpowers to impose decisions but, rather, in favor of blocking nonimplementable decisions from being taken. In response to the New Zealand delegate, who expressed concern over the excessive power of the permanent council members, the British delegate argued (and was often cited when tackling nonpermanent members' protests against the veto power) that all the permanent members together cannot carry anything against five nonpermanent members (Russell and Muther 1958). Thus the aggregate power of the five superpowers in the council was deemed no greater than that of any other five nonpermanent members. In a purely formal sense, this assertion is correct, since the permanent council members do not constitute a majority. As will be shown, in practice, this argument is misleading and deceptive.

This paper uses noncooperative bargaining theory to explore the consequences of veto power in committees. Bargaining theory was used by Baron and Ferejohn (1989) to examine legislatures operating under majority rule, that is, without veto power. Baron (1989) explored the effect of coalition discipline on the size of the coalition in majority legislatures using a bargaining approach, and Harrington (1990) examined the first-proposer advantage, again in a majority legislature, as a function of the voting rule. In contrast to this literature, my objective is to explore the role of veto power and to study institutional measures for reducing it. In particular, I am interested in the effect of the length of negotiations (the deadline) on the power of veto players, as well as the effect of expanding the committee to include more nonveto players. Furthermore, the bargaining model I use differs from that used traditionally in the literature, as it deals with a class of bargaining games rather than a specific protocol.

Bargaining is perhaps the main tool by which committees reach decisions. Committee members often reach agreement through negotiation long before voting takes place, and sometimes voting merely formally ratifies these agreements. This preliminary phase of collective decision making has its own rules and conventions, which are often fairly strict and precise, and they allow for a rigorous game-theoretic analysis.

The first result introduced here, which is a benchmark result (proposition 1), asserts that, from the point of view of bargaining theory, it is the veto players who extract all the gains from the collective decision made by the committee. This seems to support the concern expressed by nonpermanent members of the Security

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[1] See Russell and Muther (1958) for further discussion.

Council regarding their comparative importance within the committee. Following proposition 1, I explore three effects of reducing veto power. It will be shown that when delay is costly, the power of nonveto players increases. A similar effect can be achieved by imposing an exogenous deadline for the negotiation. It also will be shown that the equilibrium payoff of each nonveto player decreases monotonically with the length of the horizon of the negotiation.

During the first three decades of the United Nations, various resolutions were adopted by the General Assembly aimed at diminishing the excessive power of permanent council members. In November 1947 an interim committee was established to investigate the problem of voting in the Security Council. On April 14, 1949, the General Assembly set forth a list of 35 types of decisions that were to be deemed procedural and therefore beyond the scope of the veto. Perhaps the most drastic measure adopted by the assembly with respect to this issue resulted in the expansion of the council to include four more nonpermanent members, while preserving the veto power of the permanent members. The effect of this measure will be examined in a later section, showing that expanding committees in the manner described above indeed reduces veto power, but that such a goal can be achieved only in the framework of finite-horizon negotiations (i.e., a finite, predetermined deadline).

THE MODEL

Committees

A group of n agents is involved in a decision-making process within a committee on which there are veto players. The committee is formally described by a voting game (N,W), where N is the set of committee participants, and W is the collection of all winning coalitions (subsets of N). Intuitively, W is the set of all the coalitions that can enforce a decision within the committee. A participant has a veto right if s/he is a member of any winning coalition. Hence, $T = \cap_{S \in W} S$ is the set of all veto players and is assumed to be nonempty. Any player in T can block a decision, although T itself need not be winning. By imposing a decision, any winning coalition can earn one unit of utility, which can then be divided among its members. Intuitively, every division of one among the members of the committee is assumed to correspond to the utility vector attained by some possible decision.

In addition to the existence of veto players, the following standard assumptions are made regarding the structure of the committee: (1) W satisfies monotonicity (i.e., if $R \in W$ and $R \subset S$, then $S \in W$); and (2) there is no dummy player (i.e., $N = \cup_{S \in W^m} S$, where W^m is the set of all minimal winning coalitions).[2]

As mentioned earlier, I will use the sequential bargaining approach to analyze the process of decision making in committees and to explore the consequences of granting some members veto power. In fact, the bargaining model involves a class of games all based on the sequential submission of proposals by the committee members regarding the division of the unit of utility. Each game in this class is characterized by some probability distribution that determines the forum of negotiations at each period of the game.

The Bargaining Game

Bargaining is carried out according to the following procedure. A winning coalition is chosen randomly according to some probability distribution over all winning coalitions, that is, for each $S \in W$, there exists a positive probability $P(S)$ that S will be drawn and $\Sigma_{S \in W} P(S) = 1$.[3] After some $S \in W$ is chosen, a player i in S is chosen randomly according to a uniform probability distribution over all players in S. Player i introduces a proposal of how to divide the one unit of utility between the members of the winning coalition S. Thus, a proposal is a vector $(x_1, \dots, x_s)$ $s = \#S$, where $\Sigma_{j \in S} x_j \leq 1$. After introducing a proposal, the remaining players in S respond to i's proposal in some order. A responding player can either accept a proposal or reject it. If all the players in S accept i's proposal, it is implemented, and the game ends with a payoff of x_j for j in S and 0 for j in $N \backslash S$. If some player in the coalition S rejects the proposal, the coalition S dissolves, the bargaining moves on to a new stage, and the game continues in the same manner until either an agreement is reached or perpetual disagreement prevails. In the latter case, all the players receive zero payoff.

The approach used here is somewhat more general than the usual approach to bargaining theory because it allows for simultaneous treatment of a *class* of bargaining mechanisms instead of a single procedure. The random mechanism that determines the potential coalition at each stage should not be taken literally. Committee negotiations in real life do not involve tossing a coin before proposing. What this feature tries to capture is the fact that the bargaining protocol can be amorphous. It reflects the idea that the probability of a coalition to negotiate an agreement may depend on elements outside the model, possibly alliances or rivalries on other issues, which may be very relevant in the case of the Security Council. The way coalitions emerge in order to negotiate an agreement may be quite complicated. A coalition may emerge through an initiative by one of its members. But, especially in informal negotiations, there may be other ways. The feature of random coalitions allows us to compress this ambiguous procedure into one probability distribution over coalitions. Note that although potential coalitions emerge randomly in the model, successful coalitions are still generated *endogenously* (through acceptance of proposals). Another observation captured by this model is the fact that serious negotiations involve only winning coalitions, since only these can enforce a decision. As will be argued later, the results are robust even beyond the class of games specified above. In particular, they survive in a setup without random coalitions.

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[2] S is minimal winning if it is winning and no strict subset of it is.

[3] All the results hold true also for probability distributions on the domain of all minimal winning coalitions.

The bargaining procedure described previously induces an extensive form game with an infinite horizon. In what follows I focus on the equilibrium outcomes of this game. The basic game-theoretic solution used to analyze the bargaining game is subgame perfect equilibrium. I confine my attention here, however, to equilibria with some weak stationarity property (explained below) in order to get a meaningful result and avoid the "Folk Theorem" phenomenon of models of multilateral bargaining (namely, that every outcome can be supported as an equilibrium; see, e.g., Sutton 1986 and van Damme, Selten, and Winter 1990).

The (subgame perfect) equilibria of interest are those in which players use strategies that are history dependent in only a limited way. Specifically, on the one hand, a proposer can make a proposal dependent only on the period of the game (and, of course, on the relevant coalition) but not on previous proposals and responses. On the other hand, responders' actions are functions of the period of the game, the relevant coalition, the proposal, and even the name of the proposer but cannot depend on earlier proposals and responses. Note that this notion of stationarity is weaker than the usual stationarity property often used in the literature on multilateral bargaining, in that it allows players to use a "clock," namely, to make their actions dependent on the period of the bargaining. Thus, this notion allows for more equilibria than the standard stationarity assumption. Bear in mind that the stationarity assumption imposes no restriction on players' strategy sets, and players cannot improve by deviating from such equilibria, even if they use nonstationary strategies. I will henceforth use the term "equilibrium" when referring to subgame perfect equilibria with the above stationarity property.

VETO OR DICTATORSHIP?

The first result characterizes the equilibrium outcomes of the bargaining games described in the previous section. Proposition 1 establishes that the utility induced by the decision making in the committee accrues only to the veto players; the interests of nonveto players are virtually ignored, as their share of the benefits is zero.[4] Formally:

Proposition 1: *At any equilibrium each nonveto player earns zero expected payoff, and the unit of utility is divided between the veto players. Furthermore, any allocation of the unit of utility among the veto members is sustainable in equilibrium.*

Note that the nonveto players' payoff is zero regardless of the probability distribution $\{P(S); S \subset N\}$ according to which coalitions are drawn, as long as these probabilities are positive. Since all veto players are always members of the coalitions formed, this also means that the set of equilibrium outcomes is independent of this probability distribution.

In practice, members who enjoy veto power gain much more than the ability to block unfavorable decisions. Collectively they can, in fact, impose any decision they wish, even if they have no formal majority (i.e., if the coalition of all veto players is not winning). The excessive power of permanent Security Council members is often acquired by repeated vetoes on the same issue. Bailey (1969) has concluded that out of the 109 uses of veto between 1946 and 1967, only 66 related to original proposals, and the rest were vetoes of amended proposals that had already been vetoed.[5]

The core of a voting game with veto players consists exactly of those payoff vectors in which the unit of utility is divided in some manner among the veto players. Proposition 1 shows that the set of equilibrium outcomes of the bargaining game coincides with the core of the voting game. The literature on the relation between the equilibria of noncooperative bargaining games and the core of coalition form games has recently been growing.[6] It is beyond the scope of this paper to review that literature, especially as I wish to focus on measures for reducing veto power in committees. Furthermore, none of the results offered in that literature is applicable for proposition 1 because of the special nature of my bargaining procedure and the weak stationarity notion imposed.[7] The special feature of the model described here is the random mechanism, which is responsible for convening coalitions. This mechanism guarantees that no winning coalition can be excluded. In all the models used in the literature on core implementation, the choice of coalition is part of the proposer decision. Proposition 1 will not hold true in any such model unless one introduces further restrictions on the equilibria (as usually done in the literature).

A consequence of proposition 1 is that all equilibrium outcomes are Pareto efficient. It can be shown that if one also considers nonstationary equilibria, Pareto efficiency may fail to hold, as shown in Example A.1 in Appendix A.[8] It also should be noted that the efficiency of stationary equilibrium outcomes is driven by the special property of committee games. For a general coalition form game, a stationary equilibrium outcome may be inefficient (see, e.g., Seidmann 1989 for a similar phenomenon in a different three-person bargaining game).

DISCOUNTING

One cause of the oligarchic power of veto players in committees, as shown in proposition 1, is the fact that delay is costless. Roughly, an offer whereby some nonveto player gets a positive payoff cannot be accepted by the veto players because they would prefer to wait until a blocking coalition is formed, with the right to propose a Pareto improvement. Eventually, such an event will occur with probability of one. If waiting is costly, or if the players are impatient, this argument is no longer valid. With costly delays, each player in the committee earns a positive expected payoff in equilibrium, and unless everyone has veto power, the total payoff of the veto players is less than one. This is shown formally in proposition 2. The "discounted" version presented here differs from the original model only in that players' utility from an agreement also depends on the period in which the agreement is reached. Specifically, if it is agreed in period t that i is to receive x_i, then i's utility is $x_i\delta^{t-1}$, where δ is the discount factor.

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[4] A special case of this phenomenon (in legislative actions involving Congress and the president) was indicated independently by Diermeier and Myerson (1994).

[5] Chapter 4, p. 27.

[6] See, for example, Moldovanu and Winter (1995), Perry and Reny (1994), Serrano (1992), and others.

[7] Recall that I allow strategies to be time dependent. The fact that the core emerges with the simple models presented here depends on the special structure of voting games in the class of all coalition form games.

[8] Roughly, with complex strategies, players may use "punishments" to deter rejections. This may induce responders to accept inefficient allocations.

Proposition 2: *In the bargaining game with discount factor $\delta < 1$, every player $i \in N$ earns a positive expected payoff in equilibrium, and thus the total payoff for the veto players is less than one unless all players have veto power.*[9]

Proof. Let $v_i(t)$ be player i's equilibrium payoff in the subgame starting at period t, and let $v_T(t)$ be the total payoff for the veto players. I will in fact show a stronger result, namely, that $v_i(t) > 0$ for all players, and $v_T(t) < 1$. First, take $t = 1$. Consider some equilibrium b, and take some player i in N. For $S \in W$ with $i, j \in S$, let $v_i(1, S, j)$ be i's equilibrium payoff when S is drawn and j is chosen to propose at period 1. I argue that $v_i(1, S, i) > 0$. Suppose that $v_i(1, S, i) = 0$ for some S, and then consider the proposal by i which gives each j in $S\backslash\{i\}$ $\delta v_j(2) + \varepsilon$. Obviously, this proposal must be accepted by all the members of $S\backslash\{i\}$ (rejection yields $\delta v_j(2)$ for $j \in S\backslash\{j\}$). Player i's payoff will then be $1 - \delta \sum_{j \in S\backslash\{i\}} v_j(2) - (|S| - 1)\varepsilon$. Since $\sum_{j \in S\backslash\{i\}} v_j(2) \leq 1$, player i's payoff is positive for small enough ε. This contradicts the fact that b is an equilibrium. Note, now, that $v_i(1)$ is a weighted average of $v_i(1, S, j)$, which are all nonnegative, and some are positive. Thus, $v_i(1)$ must be positive. Since the total payoff is bounded by 1, we have $v_T(1) < 1$, whenever T is a strict subset of N. Finally, for other bargaining periods use the same argument as above by substituting periods 1 and 2 with t and $t + 1$, respectively. *Q.E.D.*

It can be shown that with a stronger notion of stationarity, that is, if players' behavior is also independent of the period of bargaining, there exists a unique equilibrium. Moreover, the equilibrium payoff for veto players increases with the discount factor; veto players always gain more than do nonveto players; and as δ approaches one the total payoff of nonveto players approaches zero. This issue is treated in Appendix A.

DEADLINES REDUCE VETO POWER

We have seen that impatience on the part of committee members acts in favor of nonveto players. Yet, as a mechanism to reduce the oligarchic power of veto members, proposition 2 is not very useful; impatience or waiting costs are hard to induce and to control. Consequently, a more useful approach is to consider procedural restrictions serving as a mechanism reducing veto power. This can be achieved by imposing a deadline on the bargaining process. Proposition 3 asserts that when a deadline is imposed, the (expected) equilibrium payoff for every nonveto player will be positive. Moreover, a long bargaining horizon operates in favor of the veto players.

I will now consider the finite version of the bargaining model described in the last section. This bargaining procedure follows the same rules as before, but if no agreement is reached within the first τ stages of the game, then the game terminates, and each player is assigned a zero payoff. The exogenous deadline τ is finite and is assumed to be commonly known. I denote by Γ_τ the bargaining game in which τ is the deadline.

Proposition 3: *For each deadline τ, the game Γ_τ has a unique subgame perfect equilibrium.*[10] *Moreover, this equilibrium satisfies three properties.*

(1) *Each committee member has a positive expected equilibrium payoff, and hence the total equilibrium payoff of all the veto players is always strictly smaller than one.*
(2) *For each veto player* i, *the equilibrium payoff is monotonically nondecreasing in τ.*
(3) *As the deadline approaches infinity, the equilibrium payoffs approach the payoff vector that divides the one unit equally among the veto players (leaving zero to all other players).*

The excessive power of veto players in bargaining without deadline is inherent in the fact that, by waiting long enough, they can be assured any winning coalition will emerge at some stage. Thus, an offer in which veto players get less than the unit of utility will be rejected by some veto player, who prefers to wait for "better" circumstances (coalition) that will increase his/her payoff. In bargaining *with* a deadline, the story is completely different. A rejecting veto player can no longer be sure that a better proposal will be made before the deadline is reached. This induces veto players to accept offers that would be unacceptable in an infinite-horizon bargaining process and increases the equilibrium payoff of nonveto players. This is the intuition underlying proposition 3.

Interestingly, it seems that UN members in the early days understood the effect of lengthy negotiations on the power of permanent members. In December 1946 the General Assembly adopted resolution 40 (1), which was initiated by the protest of nonpermanent council members against the voting privileges of permanent members. Among other things, this resolution stated:

> The General Assembly earnestly requests the permanent members of the Security Council to make every effort, in consultation with one another and with fellow members of the Security Council, to ensure that the use of the special voting privilege of its permanent members does not impede the Security Council in reaching decision promptly.

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[9] Hereafter, "equilibrium payoff" will be used to indicate expected payoff before a coalition is drawn.

[10] In the finite horizon framework, no stationarity is needed.

The emphasis on promptness (as opposed to effectiveness) in a resolution designed to respond to nonpermanent members' concern about the veto power indicates that UN members were aware of the relation between the length of negotiations and the distribution of power within the Security Council.

I conclude this section by showing that even though nonveto players benefit from a short deadline, their equilibrium payoff can never exceed that of a veto player.[11]

PROPOSITION 4: *For every bargaining horizon* τ, *the equilibrium payoff of every veto player* $i \in T$ *is greater than the equilibrium payoff of every nonveto player* $j \in N \backslash T$.

Proof. Using proposition 3, it is enough to show the assertion for $\tau = 1$.[12] Let $i \in N$, and suppose that the coalition S is drawn and that $i \in S$ is chosen to propose. If the bargaining runs for one period only, the resulting game is an ultimatum bargaining game. Player i will thus propose the whole unit of utility for him/herself, and this proposal will be accepted by the rest of the players in S (in subgame perfect equilibrium). Hence, for $\tau = 1$ the equilibrium payoff for each i in N equals the probability that i is called to propose. This probability is given by

$$P_i = \sum_{S \in W} P(S) \frac{1}{|S|} \text{ for } i \text{ in } T, \text{ and by } P_j = \sum_{\substack{S \in W \\ j \in S}} P(S) \frac{1}{|S|}$$

for nonveto players. Since $P_i > P_j$, the result is established.[13] *Q.E.D.*

THE EFFECT OF EXPANDING COMMITTEES

In 1965 a measure was taken to reduce the excessive power of the permanent members of the Security Council. Under pressure exerted by nonpermanent members it was decided that the council should be expanded by four nonpermanent members. The Security Council now consisted of the same five superpowers (permanent members) and ten nonpermanent members. The new voting majority was set at nine out of fifteen instead of seven out of eleven, but the five permanent members retained their veto power. Note that in the framework of bargaining without a deadline (infinite horizon), this reform should have no effect whatsoever on the equilibrium payoffs of nonpermanent members. This is because proposition 1 does not depend on the precise structure of the committee. As long as there is at least one veto player, all nonveto players get zero in equilibrium. Proposition 5 shows that when bargaining has a finite horizon, expanding the committee in the manner described above will always result in reducing the power of the veto players.

Note that two opposing forces operate when the committee is expanded. On the one hand, it becomes harder for veto players to form a winning coalition because that requires the supporting votes of more nonveto members. On the other hand, the power of nonveto members may be reduced since each such member now has more substitutes than before. It turns out, as proposition 4 shows, that the first force is stronger than the second. That is, for any given horizon, the power of every veto player decreases as a result of expanding the committee.

To make this comparison, I will consider committees in which a winning coalition consists of the veto players and a majority of its members (not necessarily a simple majority). Let C_1 be such a committee with n_1 members and a set T of veto players. I will say that the committee C_2 is an expansion of C_1 if (1) C_2 has the same set of veto players T as in C_1; (2) C_2 uses the same majority rule as C_1; and (3) C_2 has n_2 members, where $n_2 > n_1$. Note that this is exactly the sense by which the Security Council after 1965 is an expansion of the former structure.

For the sake of simplicity, I will restrict myself in proposition 5 to probability distributions $\{P(S); S \subset N\}$, which are uniform on the domain of all minimal winning coalitions.[14] Namely,

$$\forall S \in W^m P(S) = \frac{1}{|W^m|}, \text{ and } \forall S \notin W^m P(S) = 0.$$

PROPOSITION 5: *Consider two committees,* C_1 *and* C_2*, such that* C_2 *is an expansion of* C_1*. For each veto player* i *in* T *and for each bargaining horizon* τ*, player* i*'s equilibrium payoff in* C_2 *is lower than his/her equilibrium payoff in* C_1.

Suppose that a deadline is imposed in order to bound the total payoff of veto players somewhere away from one. What proposition 5 says is that expanding the committee affords a longer bargaining horizon to attain the same bound. Nevertheless, no matter how much the committee is expanded, the payoff for a nonveto player can be driven down to almost zero by allowing a sufficiently large horizon. Hence, the objective of reducing veto power cannot be achieved simply by expanding the committee. This is perhaps the theoretical reason for the Riker and Ordeshook (1973, 172) claim that, in reality, "this reform [in 1965] accomplished nothing."

Table 1 displays the equilibrium payoffs for veto and nonveto members of the Security Council before and

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[11] I would like to thank one of the referees for the suggestion to incorporate this result in the paper.

[12] If all nonveto players are symmetric, this follows directly from the fact that the equilibrium payoff of the veto players is nondecreasing in the length of the game. If not, all veto players are symmetric. Use proposition 3.1 with similar arguments as in proposition 3 to conclude that the equilibrium payoff of each nonveto player is nonincreasing as a function of the length of the game.

[13] Once a coalition is selected, the probability of proposing first is the same for veto and nonveto players. Nevertheless, ex-ante, before a coalition is drawn, veto players have higher probability to propose first. This fact drives the higher payoff for veto players in a *single* bargaining period. Yet, the fact that veto players maintain their higher payoff for *any* horizon is driven also by their other advantage, namely, only veto players are members of the drawn coalition with certainty (see proposition 3). In the sequel, a different model is discussed, in which the first-mover advantage plays no role, but proposition 4 still holds.

[14] In fact, the result holds for any probability distribution (not necessarily the uniform) provided that one imposes a restriction on the probability distribution used for the extended committee. A sufficient condition, for example, is that the probability of being a member of the drawn coalition in the extended committee does not exceed the same probability in the original committee.

TABLE 1. Equilibrium Payoffs for Veto and Nonveto Players of the Security Council, Before and After 1965

	Length of Negotiation											
	$\tau = 1$		$\tau = 2$		$\tau = 3$		$\tau = 4$		$\tau = 5$		$\tau = \infty$	
Period	Veto	Nonveto	Veto	Nonveto	Veto	Nonveto	Veto	Nonveto	Veto	Nonveto	Veto	Nonveto
Before 1965	0.1428	0.048	0.1700	0.0250	0.1842	0.0131	0.1917	0.0070	0.1960	0.0036	0.2	0
After 1965	0.1111	0.0444	0.1400	0.0296	0.1578	0.0211	0.1720	0.0140	0.1820	0.0090	0.2	0

after 1965 as a function of the length of negotiation.[15] These values were calculated for the uniform probability distribution over the domain of minimal winning coalitions using equation 4 in Appendix B recursively. For $\tau = 1$, for example, the payoff for each veto player is the probability of being the first proposer, which is one in eleven (before 1965). This, together with equation 4, yields the payoff for $\tau = 2$, and so forth. Note that after the expansion the payoff for nonveto players converges to zero at a slower pace. Moreover, not only does each veto member's payoff decrease after expanding the committee, but also for $\tau \geq 2$ each nonveto player strictly gains from the expansion, despite the fact that the unit of utility is now shared among a larger group of players.

AN ALTERNATIVE MODEL OF BARGAINING

To analyze the effect of the bargaining horizon on veto power, I have used a class of games in which potential coalitions emerge randomly. I have discussed the rationale behind the model of random coalitions. Yet, the results of this paper are robust to the bargaining protocol even beyond the original class of games. I will now discuss an alternative model which does not involve random coalitions and which is similar to the models used by Baron and Ferejohn (1989) and Harrington (1990) for majority legislatures. First, at the beginning of a period, a player is chosen randomly (with uniform probability distribution) to propose. A proposal is now an allocation $(x_1, x_2, \ldots, x_n)$ of benefits among *all* players. Second, according to some order (or simultaneously) all players respond by either accepting or rejecting. (This can be thought of as a formal voting.) Let S be the coalition of all accepting members. If S is winning, the game terminates by implementing the proposed allocation. If S is not winning, the game moves to the next period by drawing a new proposer. Third, again, if no agreement is reached within the given deadline, then all players earn zero (in the infinite-horizon version only indefinite disagreement leads to zero payoffs). Note that this model represents better the scenario of formal discussion before the voting takes place rather than the more ambiguous negotiations behind the scene.[16]

Table 2 displays the equilibrium payoffs of the alternative model before and after 1965.

It can be shown that all the results of this paper hold true with respect to this alternative model. In particular, the main result, which asserts that veto power diminishes as the deadline becomes shorter, is shown in a similar way as in proposition 3 by using the following recursive equations:[17]

$$v_i^1 = 1/n; \text{ and } v_i^t = \frac{1}{n}\left(1 - \sum_{\substack{j \in S \\ j \neq i}} v_j^{t-1}\right) + \frac{n-1}{n} p\, v_i^{t-1},$$

where v_i^t is i's equilibrium payoff at the subgame starting t periods before the deadline, S is a minimal winning coalition, and p is the probability that player i is offered a positive payoff.[18] Note also that the convergence to the absolute veto power is much slower here than in the original model with random coalitions. This is due to the fact that now proposers consider offers to *all* players. Figures 1 and 2 compare veto equilibrium payoffs for the two types of models (with and without random coalitions).

OTHER ENVIRONMENTS WITH VETO POWER

Apart from the UN Security Council, many other institutions grant veto power to some of their decision makers, either formally or informally. One example, which is perhaps most often discussed in the literature, is the forum consisting of the U.S. president and Congress. The president's veto power on congressional decisions is granted by the U.S. Constitution, and its consequences have been studied extensively by political scientists. Brams (1986) has used the Banzhaf and Johnston index to compute presidential power in the 536-person game comprising Congress and the president. Diermeier and Myerson (1994) have elegantly used the Shapley-Shubik value to show that by granting some congressional members veto powers, Congress can increase its power against the president as an external veto player. They

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[15] Some cooperative indices for the Security Council before 1965 yield the following: the Shapley Shubik index—0.197 for veto players and 0.0021 for nonveto; the Banzhaf and Johnston index—0.181 for veto and 0.016 for nonveto; and the Nucleolus—0.2 for veto players and 0 for nonveto.

[16] Dropping the requirement of positive probabilities in the original setup makes this model a special case within the original class of games, that is, the one with probability one for the grand coalition.

[17] In this game proposers have many actions among which they are indifferent. Thus, subgame perfect equilibrium is no longer unique. But there is a unique subgame perfect equilibrium in which players assign equal probabilities to actions among which they are indifferent.

[18] The recursive equations also yield propositions 4 and 5 using basically the same proof strategy as with the original model, and noting that $p = 1$ for veto players and $p < 1$ for nonveto players. The proof of proposition 1 makes use of lemma 2 (see Appendix B).

TABLE 2. Equilibrium Payoffs for Veto and Nonveto Players of the Security Council, Before and After 1965 (a model without random coalitions)

	Length of Negotiation											
	$\tau = 1$		$\tau = 2$		$\tau = 3$		$\tau = 4$		$\tau = 5$		$\tau = \infty$	
Period	Veto	Nonveto	Veto	Nonveto	Veto	Nonveto	Veto	Nonveto	Veto	Nonveto	Veto	Nonveto
Before 1965	0.09	0.09	0.124	0.0633	0.147	0.0441	0.163	0.0307	0.1742	0.0215	0.2	0
After 1965	0.0666	0.0666	0.0933	0.0533	0.1147	0.0426	0.1317	0.0341	0.1454	0.0273	0.2	0

also have shown that such an incentive to create an internal veto power is even stronger in bicameral legislatures, that is, a game comprising the Senate, the House, and the president. Recently, McCarty and Poole (1995) introduced an interesting empirical analysis exploring the presidential veto power on legislative actions. They mentioned that between 1789 and 1992 presidents used their veto power more than 1,400 times, and in only less than 7% of these cases was the veto overridden.

Committees that operate under a complex voting procedure may grant members an implicit veto power. Indeed, this is often the case when the criterion for being a winning coalition is more restrictive than simple majority or even qualified majority. The rule for amending the Canadian Constitution is a good illustration. Between 1867 and 1982, the British Parliament had formal veto power over any proposal for amending the Canadian Constitution. In 1982 the British veto was replaced with a rule requiring that the proposal be supported by at least two-thirds of the provinces in Canada *and* that the supporting provinces comprise at least 50% of the population. In that year the two major urban provinces, Ontario and Quebec, constituted 34.85% and 28.94% of the Canadian population, respectively, and the new rule granted them (as a single player) veto powers without making them a winning coalition. A similar phenomenon also led Brams, Doherty, and Weidner (1994) to conclude that Germany and France have de facto veto power over many issues discussed within the institutions of the European Community.

The presence of complex rules such as the one described above raises an interesting strategic question regarding the possibility of a group of players merging to become a single player. If some players' interests are relatively identical, then they may be inclined to merge and present themselves as a single player. As the results presented here indicate, if such a merger grants them an implicit veto power, then their total benefits will substantially exceed their proportion in the committee.[19] Examples of such a strategy being used often occur in disputes between creditors on the division of the remaining assets of a bankrupt firm. In many countries the relevant legislation requires that in case of a bankruptcy a

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[19] More precisely, suppose that (N,v) is a weighted majority game $[q; w_1, \ldots, w_n]$ without veto players. If players i and j become a veto player by merging, then for a long enough horizon, their equilibrium payoff v_{ij} in the game in which they operate as a single player will exceed their relative weight, i.e., $v_{ij} > (w_i + w_j)/\Sigma_{k \neq ij}\, w_k$. This follows immediately from proposition 3.

FIGURE 1. Equilibrium Payoffs for Veto Players with Random Coalitions (Dotted Line) and without Random Coalition (Solid Line), before 1965

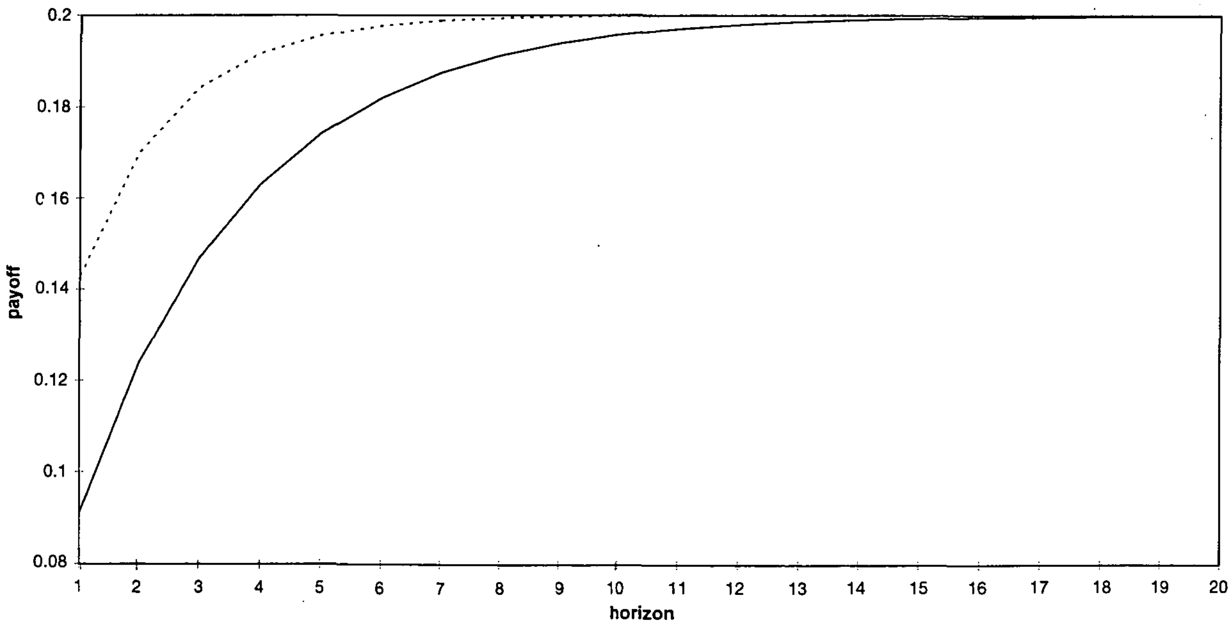
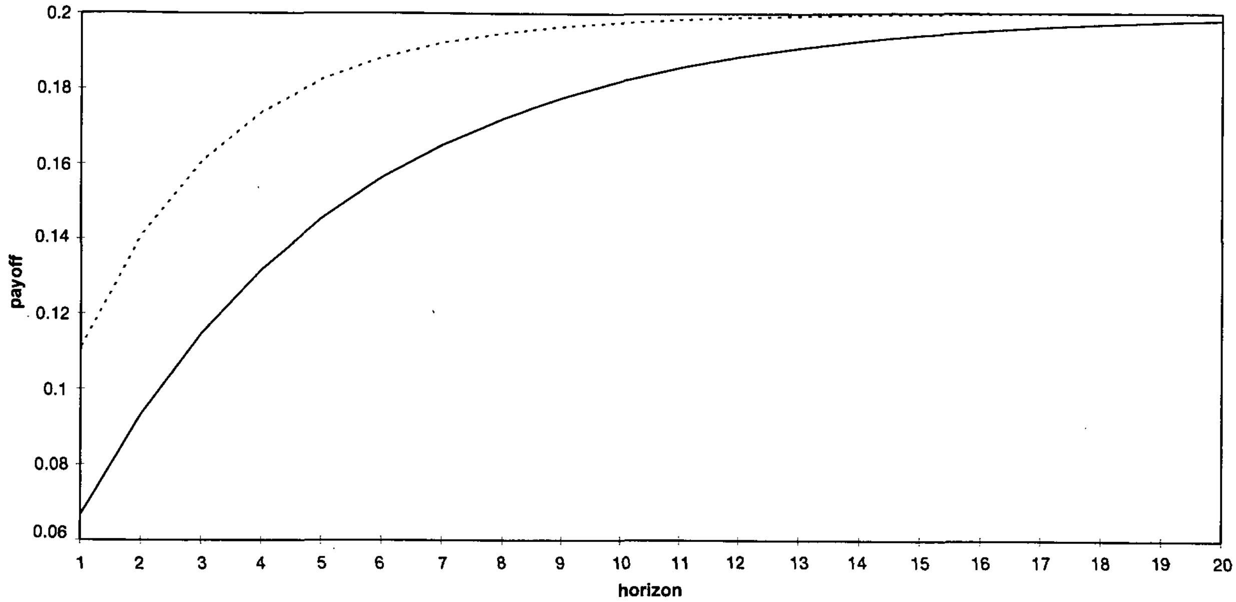


FIGURE 2. Equilibrium Payoffs for Veto Players with Random Coalitions (Dotted Line) and without Random Coalition (Solid Line), after 1965



decision by the creditors must be supported by a majority (or a qualified majority) of creditors, and these must account for at least half the debt. Depending on the distribution of the debt among the creditors, this often initiates either a buy out of one creditor's debt by another or the merger of several creditors into a single one. Finally, another interesting example of formal veto power, from the literature in finance, is that of "golden share." The holder of a golden share in a corporation not only shares the ownership of the firm but also is granted a veto power in shareholders' meetings (at least on certain issues). In most cases the firm is a former state enterprise that has been privatized. As the seller, the state may condition the deal on holding a golden share, with which its representative can veto shareholders' decisions to protect the public interest.

CONCLUSION

It has been shown that from the point of view of noncooperative bargaining theory, the bargaining power of the nonveto members is effectively null in committees in which some players have a veto power. This excessive power of veto members can be reduced if delay is costly or if an exogenous deadline for the negotiations is imposed.[20] I have also shown that the total bargaining power of the veto members increases with the length of the bargaining horizon and converges to "absolute" power as the horizon goes to infinity. Finally, I explored the effect of expanding the committee by adding more nonveto players, as was actually done by the UN Security Council in 1965. If a bargaining deadline is exogenously fixed, the bargaining power of veto members decreases as the committee expands, but such expansion has no effect whatsoever unless a deadline is also set. In practice there are various ways in which deadlines can be imposed. The most common is to set a date by which a decision has to be made; or one can set a deadline in terms of the number of meetings the committee holds on a given issue. As indicated earlier, however, permanent members of the Security Council acquire their excessive power by vetoing proposals repeatedly on the same issue. This suggests that the simplest and most effective way to diminish the veto power without abolishing it altogether is to restrict the number of vetoes each permanent member is allowed to submit on the same issue.

APPENDIX A

Proposition A.1 concerns committees in which all nonveto members are symmetric, as in proposition 5.

PROPOSITION A.1. *For every discount factor* $0 \leq \delta < 1$, *and for every equilibrium of the corresponding discounted bargaining game, the equilibrium payoff of every veto player* $i \in T$ *is greater than the equilibrium payoff of every nonveto player* $j \in N \backslash T$.[21] *Furthermore, the payoff for each veto player increases with* δ, *and as* δ *approaches 1 the equilibrium outcome approaches an equal share of the unit of utility among the veto players* only.

Proof. Let r be the number of veto players in the committee, and suppose that a minimal winning coalition consists of the veto players and m nonveto players. Denote by q the probability of each veto player being a proposer at the beginning of a period (i.e., $q = 1/(m + r)$). I also denote by k the number of minimal winning coalitions. With these notations, and by setting v_i to be i's equilibrium payoff, we obtain

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[20] Obviously, with both effects together one can reduce the excessive power of veto players even more. Bargaining with exogenous deadline and discounting, however, may induce inefficiencies, as was shown by Fershtman and Seidmann (1993) in a two-person bargaining game.

[21] By equilibrium we mean subgame perfect equilibrium with the stronger stationarity property (see the section on discounting).

$$v_i = \sum_{S \in W^m} \frac{1}{k} q\left(1 - \sum_{\substack{j \in S \\ j \neq i}} \delta v_j\right) + (1-q)\delta v_i$$

for veto players, and

$$v_i = \sum_{\substack{S \in W^m \\ i \in S}} \frac{1}{k}\left[q\left(1 - \sum_{\substack{j \in S \\ j \neq i}} \delta v_j\right) + (1-q)\delta v_i\right]$$

for nonveto players. It is a standard matter to verify that these equations are linearly independent. Let q_1 be the probability of a specific nonveto player being the proposer and q_2 be the probability of a specific nonveto player being a member of the drawn coalition *but* not being chosen to propose. Because of the symmetry among the nonveto players, these equations imply the following two:

$$v_T = q(1 - (r-1)\delta v_T - m\delta v_{-T}) + (1-q)\delta v_T$$

and

$$v_{-T} = q_1(1 - r\delta v_T - (m-1)v_{-T}) + q_2\delta v_{-T},$$

where v_T is the equilibrium payoff for veto players and v_{-T} is the equilibrium payoff for nonveto players. Rearranging, we obtain

$$v_T = \frac{q(1-\delta m v_{-T})}{1-\delta+q\delta r} \text{ and } v_{-T} = \frac{q_1(1-\delta r v_T)}{1-q_2\delta+q_1(m-1)\delta}.$$

Solving for v_T and v_{-T} (as a function of δ) we obtain:

$$v_T = \frac{q}{q\delta r+1-\delta} \text{ and } v_{-T} = \frac{q_1(1-\delta)}{[1-\delta(q_1+q_2)][q\delta r+1-\delta]}.$$

Since $q_1 + q_2 < 1$, $q_1 < q$, and $q < 1/r$, we have $v_T > v_{-T}$ as needed. Note also that from the solution for v_T and v_{-T} we obtain the following results: As δ approaches zero, v_T and v_{-T} approach p and q_1, respectively, that is, the probability of being a proposer. As δ approaches one, v_T approaches $1/r$ and v_{-T} approaches zero, as in the nondiscounted bargaining game. Finally, the equilibrium payoff of each veto player is increasing with δ. *Q.E.D.*

Example A.1. Nonstationary subgame perfect equilibria may yield inefficient outcomes. Consider a two-person committee in which the only winning coalition consists of both players, that is, both player 1 and player 2 are veto players. Also consider the following three auxiliary strategy combinations, $\sigma_{(1,0)}$, $\sigma_{(0,1)}$, and $\sigma_{(1/3,1/3)}$:

$\sigma_{(1,0)}$: Both player 1 and player 2 propose (1,0) when called upon to propose. Player 1 accepts an offer if and only if it yields a payoff of one. Player 2 accepts any offer.
$\sigma_{(0,1)}$: Both player 1 and player 2 propose (0,1) when called upon to propose. Player 2 accepts an offer if and only if it yields a payoff of one. Player 1 accepts any offer.
$\sigma_{(1/3,1/3)}$: Both players 1 and 2 propose (1/3,1,3). Each accepts an offer if and only if it yields a payoff of at least 1/3.

Note that $\sigma_{(0,1)}$ and $\sigma_{(1,0)}$ are both subgame perfect equilibria, but $\sigma_{(1/3,1/3)}$ is not, since each player can deviate by proposing an efficient division.

I will now construct a strategy combination τ, which is a subgame perfect equilibrium sustaining the inefficient outcome (1/3,1/3).

τ: Both players start by playing $\sigma_{(1/3,1/3)}$. If player 1 deviates from $\sigma_{(1/3,1/3)}$, both players move to play $\sigma_{(0,1)}$. If player 2 deviates from $\sigma_{(1/3,1/3)}$, both move to play $\sigma_{(1,0)}$.

The strategy combination τ is a subgame perfect equilibrium sustaining the inefficient payoff (1/3,1/3). This is so because no player will deviate from the path induced by $\sigma_{(1/3,1/3)}$, since doing so carries a penalty of getting zero, and the threat of punishment is credible (i.e., subgame perfect equilibrium).

APPENDIX B

B.1. Proof of Proposition 1

LEMMA 1. *Let* b *be a stationary subgame perfect equilibrium (SSPE). For each period* t *and for each player* i *in* N, *let* $v_i(t)$ *denote* i*'s expected ex-ante payoff at the beginning of period* t *(before a coalition is drawn), and when* b *is played. For each* $S \subset N$ *set,* $v_s(t) = \Sigma_{j \in S}\, v_j(t)$. *Then, for each veto player* i *in* T, $v_i(t)$ *is a nonincreasing function of* t, *and thus also* $v_T(t)$.

Proof. If the lemma is false, there must be some i in T and some time t such that $v_i(t) < v_i(t+1)$. Consider the following behavior by i at the beginning of time t: Regardless of the winning coalition drawn, if called upon to act, offer one for yourself, and reject any payoff that yields you less than one.

If an agreement is reached at time t, i gets one, which will never be less than $v_i(t+1)$. If no agreement is reached at time t, the bargaining moves to time $t+1$, and i's payoff at this subgame is $v_i(t+1)$. Thus, i can secure, at the beginning of period t, a payoff of at least $v_i(t+1)$, which contradicts the fact $v_i(t)$ is i's expected equilibrium payoff at the beginning of time t. *Q.E.D.*

LEMMA 2. *Suppose that* $v = (v_1, \ldots, v_n)$ *is a feasible and Pareto-efficient payoff vector (i.e.,* $\Sigma_{i \in N}\, v_i = 1$ *and* $v_i \geq 0$*), such that* $\Sigma_{i \in T}\, v_i = r < 1$.[22] *Then, there exists a winning coalition* S *such that* $\Sigma_{i \in S}\, v_i \leq 1 - (1-r)/n$.

Proof. If $\Sigma_{j \in T}\, v_j = r$, then $\Sigma_{j \in N \setminus T}\, v_j = 1 - r$. Therefore, there must exist some i in $N \setminus T$ such that $v_i \geq (1-r)/n$. Since i is not a veto player, $N \setminus \{i\}$ must be winning. Since $\Sigma_{j \in N}\, v_j = 1$, we have $\Sigma_{j \in N \setminus \{i\}}\, v_j \leq 1 - (1-r)/n$. *Q.E.D.*

Proof of Proposition 1. Lemma 1 has shown that for each j in T, the sequence $\{v_j(t)\}_{t=1}^{\infty}$ is nonincreasing. This sequence is obviously bounded below by zero, because each player can attain this payoff by rejecting all offers. This means that the above sequence has a limit. Take some i in T. For each $\varepsilon > 0$ there exists some t_ε, such that for each $t > t_\varepsilon$, $0 \leq v_i(t_\varepsilon) - v_i(t) < \varepsilon$.

Suppose, by way of contradiction, that there exists some period t^1 for which $v_T(t^1) < 1$. By lemma 1, for each $t > t^1$ $v_T(t_1) \geq v_T(t)$. It is easily shown that for every t $\Sigma_{i \in N}\, v_i(t) = 1$. Indeed, if $\Sigma_{i \in N}\, v_i(t) < 1$ it must be the case that along the equilibrium path an offer x_s with $\Sigma_{i \in S}\, x_i < 1$ is proposed and accepted with probability one. But the proposer can increase his/her payoff by allocating the remaining part of the unit of utility. By lemma 2, for each $t \geq t^1$ there exists a coalition $S_t \subset N$ such that $v_{S_t}(t) \leq 1 - \Delta$ for some fixed $\Delta > 0$. Now take $\varepsilon < \Delta/n$, and let $t^* = \max\{t_\varepsilon, t^1\}$.

Consider the following strategy for i.

(1) At each period t that satisfies two conditions — $t > t^*$ and at time t the coalition S_{t+1} is drawn (i.e., $v_{S_{t+1}}(t+1) \leq 1 - \Delta$) — i acts as follows: Propose $v_j(t+1) + \frac{\Delta}{n}$ for each j in S_{t+1}, and reject any offer that yields less than $v_i(t+1) + \frac{\Delta}{n}$.

(2) At any other time t, if called upon to act, i always proposes one for him/herself and rejects less than one.

Because of the finiteness of the set of winning coalitions, there must be a time t, reached with probability one, such that the condition $t > t^*$ holds. Since each player from the chosen coalition proposes with positive probability, the event that such a time is reached and i is chosen to propose occurs with probability one. Moreover, when such t is reached, i's proposal must be accepted. Consider the last player (say, j) to respond to i's proposal. If j rejects the proposal, j gets $v_j(t+1)$, which is less than i's offer. Continuing by backward induction on the set of responders in S_{t+1}, one obtains that i's proposal must be accepted. Now, since $\varepsilon < \Delta/n$, i obtains at this period a payoff greater than $v_i(t^*)$, which contradicts the fact that $v_i(t^*)$ is i's equilibrium payoff at the beginning of period t^*.

I now show the second assertion of the proposition. Take some payoff vector $x = (x_T, 0_{N \setminus T})$, where $x_T^i \geq 0$, $\sum_{i \in T} x_T^i = 1$, and consider the following strategy combination σ.

σ: Each player i in N proposes x_s at any decision point at which s/he has to propose in S.
Each player i in N accepts (S, y_s) if and only if $y_s^i \geq x_i$.

Obviously, σ satisfies the property of weak stationarity (the players' behavior is even independent of time). To show that σ is a subgame perfect equilibrium, note that if any player i deviates as a proposer by proposing y_s for S with $y_s^i > x_i$, then for some other player j, $y_s^j < x_j$, and j will reject the proposal (according to σ). Furthermore, if some player i rejects x_s when facing the coalition S, then in the next period the realized coalition will again contain all the veto players, and the same payoffs will be proposed. *Q.E.D.*

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[22] By feasible we also mean individually rational, i.e., $v_i \geq 0$.

B.2. Proof of Proposition 3

For each S in W, I denote by $P(S)$ S's probability of being drawn at the beginning of each stage of the game. Take some finite deadline τ, and let $v_i(\tau)$ be the expected equilibrium payoff of i at the beginning of the τth stage. In the remaining subgame there exists a unique subgame perfect equilibrium in which the player drawn to propose claims the whole unit for him/herself, and all the members of the relevant coalition accept.[23] The expected equilibrium payoff at this subgame is, thus: For i in T

$$v_i(\tau) = \sum_{S \in W} P(S) \frac{1}{|S|}$$

and for i in $N \backslash T$

$$v_i(\tau) = \sum_{\substack{S \in W \\ S \ni i}} P(S) \frac{1}{|S|}.$$

(Note that since no discounting exists, these payoffs are independent of τ.)

I now use backward induction to derive the expected equilibrium payoff at the beginning of the game. Let g be a transformation from $R^n{}_+$ to $R^n{}_+$, which is given by

$$g_i(v) = \sum_{\substack{S \in W \\ S \ni i}} P(S) \left[\frac{1}{|S|} \left(1 - \sum_{\substack{j \neq i \\ j \in S}} v_j \right) + \frac{|S| - 1}{|S|} v_i \right], \qquad \textbf{(B-1)}$$

where $v = (v_1, v_2, \ldots, v_n)$.

Take a time $t < \tau$, and suppose the subgame that starts at stage $t + 1$ has a unique subgame perfect equilibrium. Let $v(t + 1)$ be the expected equilibrium payoff of this subgame. Consider, now, stage t. If i in N was drawn to propose after the coalition S was drawn, then i has a unique optimal behavior at this decision point, namely, to propose $v_j(t + 1)$ for each $j \neq i$ in S, taking the rest for him/herself. Since each i in S is drawn with equal probability, and since the probability of S being drawn is $P(S)$, i's expected equilibrium payoff at the beginning of stage t is given by equation 1, namely,

$$v_i(t) = g_i(v(t + 1)). \qquad \textbf{(B-2)}$$

For each positive integer k, let $g^k(v)$ be defined inductively by $g^k(v) = g(g^{k-1}(v))$, and $g^1(v) = v^*$. Using the argument in the preceding paragraph, one can obtain by induction that the expected equilibrium payoff vector of Γ_τ is given by

$$v(1) = g^{\tau - 1}(v(\tau)). \qquad \textbf{(B-3)}$$

The specific assertions of the proposition can now be proved, starting with proposition 3(2). If i is a veto player, then $\Sigma_{S \in W_{S \ni i}} P(S) = 1$. We thus have

$$v_i(t) = \sum_{S \in W} P(S) \frac{1}{|S|} \left(1 - \sum_{j \in S} v_j(t + 1) \right) + v_i(t + 1). \qquad \textbf{(B-4)}$$

Since the first term in equation B-4 is nonnegative, we have $v_i(t + 1) \leq v_i(t)$ for i in T. By equation B-2, $g_i(v(t)) \geq v_i(t)$. This, together with equation B-3, shows the monotonicity in the length of the horizon.

To show proposition 3(3), note that the payoff vector v with $v_i = 1/|T|$ for i in T and $v_i = 0$ for i in $N \backslash T$ is a fixed point of the transformation g and thus must be the limit of the expected equilibrium payoff. Finally, note that because there exists no dummy player, $v^*_i > 0$ for all i in N. Using induction it can easily be shown that for each $k \geq 1$, $g^k(v^*)$ is a vector of positive values as well. For any given deadline, each player's expected equilibrium payoff is therefore positive. Since the total expected equilibrium payoff never exceeds one, the veto player must share a total payoff of less than one, and that for every given finite horizon. *Q.E.D.*

B.3. Proof of Proposition 4

Let Γ_j^τ be the bargaining game corresponding to C_j ($j = 1,2$) with bargaining horizon τ. Let $u_i^j(t)$ be i's expected equilibrium payoff in Γ_j^τ when there are t periods left until the deadline. I will show that for all $t \leq \tau$ $u_i^1(t) > u_i^2(t)$ for i in T. Translating equation B-4 to the new notation we obtain

$$u_i^j(t) = \sum_{S \in W} P(S) \frac{1}{|S|} \left(1 - \sum_{k \in S} u_k^j(t - 1) \right) + u_i^j(t - 1) \text{ for } i \text{ in } T. \qquad \textbf{(B-5)}$$

Let l_j be the size of a minimal winning coalition in C_j, and let S_j be a minimal winning coalition in C_j. Since the domain from which coalitions are drawn consists of the minimal winning coalitions, equation B-5 becomes

$$u_i^j(t) = \frac{1}{l_j} \left(1 - \sum_{k \in S} u_k^j(t - 1) \right) + u_i^j(t - 1) \text{ for } i \text{ in } T, \text{ and } j = 1,2. \qquad \textbf{(B-6)}$$

Note that $1 - \Sigma_{k \in S} u_k^j(t-1)$ is the total payoff of the $n_j - l_j$ nonveto players outside S_j. Since $\{P(S); S \in W^m\}$ is uniform on the domain, every two veto players have the same equilibrium payoff, and every two nonveto players have the same equilibrium payoff. Since the total payoff for all the weak players is $1 - |T| u_i\ (t - 1)$, we have

$$1 - \sum_{k \in S} u_k^j(t - 1) = \frac{n_j - l_j}{n_j - |T|} (1 - |T| u_i^j(t - 1)).$$

By equation B-6,

$$u_i^j(t) = \frac{1}{l_j} \frac{n_j - l_j}{n_j - |T|} (1 - |T| u_i^j(t - 1)) + u_i^j(t - 1) \text{ for } i \text{ in } T. \qquad \textbf{(B-7)}$$

Set $p_j = \dfrac{n_j - l_j}{n_j - |T|}$, and note that $p_1 \geq p_2$. Now, since $l_1 < l_2$, we also have $q_1 = \dfrac{p_1}{l_1} > \dfrac{p_2}{l_2} = q_2$. We therefore have by equation B-7:

$$\begin{aligned} u_i^1(t) - u_i^2(t) = {} & q_1(1 - |T| u_i^1(t - 1)) - q_2(1 - |T| u_i^2(t - 1)) \\ & + u_i^1(t - 1) - u_i^2(t - 1) > q_2 |T| (u_i^2)(t - 1) \\ & - u_i^1(t - 1)) + (u_i^1(t - 1) - u_i^2(t - 1)). \end{aligned} \qquad \textbf{(B-8)}$$

Since $l_2 \geq |T|$ and $0 \leq p_2 \leq 1$, we have $q_2\,|T| \leq 1$. Thus, if $u_i^1(t - 1) - u_i^2(t - 1) > 0$, then equation B-8 yields $u_i^1(t) - u_i^2(t) > 0$ for all i in T. We can now proceed by induction on the horizon, noting that for $t = 1$ we have $u_i^1(1) = \dfrac{1}{l_1} \geq \dfrac{1}{l_2} = u_i^2(1)$ and continuing with equation B-8. *Q.E.D.*

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[23] Although every player is indifferent as between accepting and rejecting, only acceptance is supported by an equilibrium. This is the same phenomenon as in two-person ultimatum bargaining, in which at the unique subgame perfect equilibrium the first player claims everything for him/herself, and the second player accepts. Rejection cannot be part of an equilibrium because this would mean that the first player must claim less than one. But no such claim can be optimal for the first bidder because s/he could raise the claim a bit and still guarantee acceptance.

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Myth and Reality in Business Support for Democrats and Republicans in the 1936 Presidential Election

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Using campaign contributions by business leaders as an indicator of their political preferences, this paper tests two different claims concerning the 1936 presidential elections: (1) Business leaders in mass-consumption industries differentially favored the Democrats for reasons of economic policy; and (2) business leaders in capital-intensive and internationally oriented industries differentially favored the Democrats for reasons of economic policy. The paper shows that in the 1936 election no industrial sector was more likely to support the Democrats when region, religion, and the special circumstances of the brewing industry (gratitude toward the Democrats for ending Prohibition) are taken into consideration. Democratic financial support came predominantly from southerners, members of the Jewish community, officers and directors of smaller businesses, and top-level business leaders from New York City.

The New Deal has recently become a major focus for social scientists concerned with power relations between the state and civil society. Much of this concern has been directed at the origins of the key legislative enactments of the early New Deal, such as the Agricultural Adjustment Act, National Industrial Recovery Act, Social Security Act, and National Labor Relations Act, about which there are wide differences of opinion among pluralists (e.g., Schlesinger 1960), neo-Marxists (e.g., Quadagno 1984), state autonomy theorists (e.g., Skocpol 1980), and non-Marxist class-dominance theorists (e.g., Domhoff 1990). Important and contentious claims also have been made about the role of business leaders in different sectors of the economy in the financing of the crucial 1936 presidential election (e.g., Allen 1991; Ferguson 1984, 1995; Fraser 1984, 1989; Overacker 1937; Webber 1991a, 1991b). The debate concerning campaign finance in that election is the empirical object of this paper.

The 1936 presidential election constituted a watershed in U.S. political history for two main reasons. First, it was one of the most striking examples of partisan realignment in this century, producing a seismic shift in electoral patterns which redefined political loyalties for a generation. It stabilized the power of the new Democratic majority coalition; made ordinary voters, not elites, the locus of political transformation; and brought significant changes in public policy (Burnham 1970; Clubb, Flanigan, and Zingale 1990; Key 1955; Nardulli 1995).

Second, as a result of the New Deal, "the parties divided on issues in 1936 as they had at no previous time this century" (Leuchtenberg 1995, 103). Divergent assumptions on the role of government, an activist welfare state versus a weak free-market state, were increasingly framed in class terms, as Franklin Roosevelt bitterly denounced what he saw as a hostile and selfish business class intent on destroying the fabric of the New Deal's attempt to help the poor and marginalized (Leuchtenberg 1963, 146–8; Wolfskill and Hudson 1969, 154–9). These "economic royalists" were seen as having made huge campaign contributions to the Republicans in order to defeat the Democratic program of reform.

In the event, Roosevelt defeated Republican Alfred M. Landon in a landslide (60.8% of the popular vote) that included large Democratic majorities in the Senate (75–16) and the House (331–88). Furthermore, Roosevelt won 76% of lower-income voters (but only 42% of upper-income voters), 81% of unskilled laborers, 80% of labor union members, 84% of relief recipients, 70–81% of Catholics, and, by the end of the decade, close to 90% of the Jewish vote (Leuchtenberg 1995, 153–6). In the opinion of most observers, in 1936 the Democrats successfully consolidated the "New Deal coalition" in spite of the unparalleled opposition of U.S. business.

Despite the general consensus on the stance taken by the business community in 1936, in recent years two major revisionist claims have asserted that some business leaders actually supported the New Deal. The first claim, championed by labor historian Steven Fraser (1984, 1989), is that business executives in the mass-consumption industries differentially supported the Democrats. The second and even more surprising claim comes from the work of Thomas Ferguson (1984, 1995), who argues that international bankers and the officers of internationally oriented and capital-intensive corporations were more likely than other types of business to support the Democrats in 1936. Both claims have been praised by academic and popular commentators alike (e.g., Brody 1985, Gourevitch 1985).

Contrary to these claims, our detailed and systematic analysis of the campaign finance data for 1936 shows that no sector of the economy was more likely to provide supporters for the Democrats when nonbusiness variables are taken into account. Instead, we find that region, religion, the end of Prohibition, and the size of a business firm are better predictors of the party preferences of most business donors. More specifically, Democratic support tended to come from southerners, Jews,

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brewers, and the officers and directors of smaller businesses, a finding compatible with studies of voting patterns in that election.

In focusing on the campaign contributions of business leaders, we are not arguing that money was the determining element in the 1936 campaign. While a certain large minimum is obviously necessary to mount a campaign, making at least some support from wealthy donors indispensable (e.g., Heard 1962, 34), the campaign with the most money does not always win (e.g., Alexander 1972). In 1936, for instance, the Republicans outspent the Democrats by $8,892,972 to $5,194,741, but the Democrats won the presidential election with 62.5% of the two-party vote nationally (Nie, Verba, and Petrocik 1976, 75; Overacker 1937, 476).

Instead, we are using individual campaign finance donations as an indicator of different business leaders' political preferences regardless of the amount of money given and its effect. In the opinion of Alexander (1972, 11), campaign donations can be seen as a "tracer element" in the political process, marking the tracks of the individual or group seeking influence. As have other quantitative studies of campaign finance, we use donations as a way to study such general issues as regional, ideological, and sectoral differences within the U.S. business community (e.g., Burris 1987; Clawson, Neustadtl, and Scott 1992; Neustadtl and Clawson 1988; Salt 1989).

DATA AND METHODS

The Federal Corrupt Practices Act of 1925 established the rules regulating the reporting of campaign finance information in the 1930s. It designated the treasurers of political parties as responsible for keeping detailed records of the names and addresses of contributors, along with the date they contributed and the amount of the contribution. Treasurers were required to submit this information on all contributors of more than $100 to the clerk of the House of Representatives throughout each presidential election year beginning on March 1, with more comprehensive reports to be submitted on January 1 of the year following the election. According to Overacker (1937, 474), the leading academic expert on campaign finance in that era, the reports were complete and accurate, especially for the Democrats. Correcting for inflation, a donation of $100 in 1936 would be equivalent to a donation of approximately $1,000 in 1995. At a time when average annual earnings were $1,184 a year and only 2.5% of American families had annual incomes of $10,000 or more, a donation of $100 was from 5–15% of total income for the great majority of U.S. families (U.S. Bureau of the Census 1975, 164, 300).

As part of her extensive research on campaign finance from 1928 to 1944, Overacker (1933, 1937, 1941, 1945) put the information provided to the House clerk for the 1936 presidential campaign on 3×5 cards. The Democratic records contained donors giving more than $100, while the Republican records reported all donors irrespective of the amount. Overacker systematically organized these names, which had been randomly entered on legal-size sheets, a task involving a tremendous expenditure of time and energy. Her records are now in the possession of the Citizens Research Foundation at the University of Southern California. At the time of writing, they remain the only organized source for the 1936 campaign finance data, and they provide the primary database for our analysis.

Although the 1925 act authorized destruction of the original reports, they still exist, at the National Archives in Washington, D.C. (National Archives 1936), and the accuracy of Overacker's records was checked by drawing a random sample of 100 names from her cards and comparing these against the original reports. We found a perfect match between Overacker's records and the original reports. Therefore, we have great confidence in the quality of her information.

To test claims concerning the financial support for Republicans and Democrats by business leaders in the 1936 presidential campaign, information was gathered on the top officers and directors of commercial banks and corporations and on the partners in investment banking firms. Their names were obtained from *Poor's Register of Directors of the United States and Canada* (1936) and two Moody's investment manuals: *Banks, Insurance, Real Estate, and Trusts* (1937) and *Industrial Securities* (1937). To gather the information on region and religion for some of our analyses, we used *Who's Who in America* (1936–37), *Who's Who in American Jewry* (1938/39), *Who's Who in New York (City and State)* (1938), and obituaries in the *New York Times*.

In making our analysis, we excluded individuals who gave before November 1934 or after the election. Any contributions made before November 1934 could have been intended for the midterm elections that year, and any contributions after the 1936 election could be interpreted as an attempt to gain access to the winning candidate. The fact that some postelection contributions to the Democrats came from people who gave to the Republicans before the election supports our exclusion of postelection contributions.

Not all campaign proceeds in 1936 came from individual donations to the candidate or party. For example, some of the Democratic revenues came from bank loans, business advertisements in a souvenir convention book, business and individual purchases of that book, and labor unions. Since our focus in this paper is on the preferences of individual business leaders making direct donations, these other sources of funds are not considered here. Nevertheless, an analysis of the loans, advertisements, and purchases of the convention book shows that including them would not alter our general conclusions (Webber 1996). In the case of the Democrats' two bank loans, from Chase Manhattan and Manufacturer's Trust, it need only be said that both were issued after Roosevelt's electoral victory, making them irrelevant even if someone were to make the unlikely argument that they could be statements of party preference.

In the case of advertisements in the Democrats' souvenir book, it is unlikely that any company's political preferences were being expressed, especially since some of these companies' officers donated to the Republicans (Overacker 1937, 480). Moreover, the issue is minor due to the fact that only 32 of the 270 largest businesses that we focus on in the final section of the paper purchased an advertisement (Webber 1996).

As for purchases of the souvenir book by companies, a case could be made that this indicates a party preference on the part of at least some company officers, but only seven of the largest businesses bought copies before the election. Among the individuals who purchased one or more copies, only twenty-three (4% of all individual purchases before the election) were connected with one of the top businesses (Webber 1996).

Although there are several ways in which nominal data such as ours can be presented and analyzed (e.g., Bohrnstedt and Knoke 1994, Reynolds 1984), we prefer to emphasize percentage differences in 2×2 tables and rely on chi square as our significance test for several reasons. First, these are as good as log linear analysis for our purposes because we are not using magnitudes of campaign contributions, only presence or absence of donations to each party, and because there is no ordering principle that would allow us to place business sectors along a continuum (e.g., Kennedy 1992, 1, 49). Second, there is reason to believe that useful chi-square values can be obtained with expected cell frequencies as low as one when the *n* is 20, without any correction for continuity (Camilli 1995; Camilli and Hopkins 1978, 1979). In addition, chi square is equivalent in a 2×2 table to a test for the significance of differences between two independent proportions; that is, $\chi^2 = z^2$ (Ferguson 1981, 211–3). Finally, the percentage data in a 2×2 table allow us to report a correlational statistic and effect sizes as well. Specifically, the Pearson *r* for dichotomous variables is equal to the percentage difference between the two cells in the top row of a 2×2 table (Rosenthal and Rubin 1982), and both the effect size phi (ϕ) and effect size lambda (λ) are also equal to the same percentage difference in a 2×2 table. That is, $r = \phi = \lambda$. Thus, we will report chi squares and Pearson *r*'s for each analysis, with the understanding that the Pearson *r* is the same as the effect sizes phi and lambda in this instance.

GENERAL FINDINGS

Before beginning our analysis of the claims by Fraser and Ferguson, three general findings need to be stated in order to provide a proper context for our specific results. First, only a small percentage of the officers and directors of the banks and corporations we examined gave $100 or more to campaign chests of either party. There were variations among business sectors, but overall only about 30% of business leaders actually contributed in 1936. This finding is similar to what Allen (1991) reports for 300 members of wealthy families and 250 top corporate executives for the same election. It is also consistent with the low percentage of business contributors found by Heard (1962, 93) for the 1956 election and by Alexander (1971a) for corporate officers and directors in the 1968 presidential election. In most of our tables, then, we will focus our analysis on those who made contributions, although in three of them we will report the total number of officers and directors we studied for each business or business sector as well.

Second, we found that virtually no business leaders gave to both political parties. The few who did, as we already stated, usually gave to the Democrats after Roosevelt won the election. This finding makes it very easy to organize the data into two-way tables.

Third, we established a baseline percentage for determining whether officers and directors in any given business sector were differentially favorable to the Democrats. To do so we drew a random sample of 3,098 donors from Overacker's records and then compared them with the alphabetical list of officers and directors in the back of *Poor's Register of Directors of the United States and Canada*, 1936. Since the list contains people affiliated with incorporated businesses ranging in size from very small to very large in all business sectors, we believe it is the best single source for establishing general business preferences. We found that 32.8% of all donors in the sample, whether in business or not, gave to the Democrats, compared to 67.2% for the Republicans, an unsurprising finding in the context of the literature on campaign finance. We also found that only 31.1% of all donors were listed in *Poor's*, which suggests that a great many donors were professionals, successful commercial farmers, or small businessmen. Most important for our purposes in this paper, only 17% of the donors found in *Poor's* gave to the Democrats. This low figure provides us with a baseline for comparisons with specific business sectors. That is, a sector need only favor the Democrats by a statistically significant percentage above 17% for it to show a differential preference for that party.

MASS-CONSUMPTION INDUSTRIES AND PARTY PREFERENCE

The first revisionist claim concerns the role of the mass-consumption industries. Fraser (1989, 60), drawing in part on the unpublished work of Friedlander (1987), asserts that the 1920s saw the emergence of "newer, mass-consumption oriented industries" coalesced in and around the Taylor Society. Originally the institutional home of scientific management, the Taylor Society drifted leftward, according to Fraser, gradually embracing collective bargaining and some notions of industrial democracy. Its supporters consisted largely of mass merchants, real estate interests, newer investment houses such as Lehman Brothers and Goldman, Sachs, and the clothing and household appliance industries. These sectors are said to be "associated with the exponential growth in the size and depth of the urban mass market" (Fraser 1989, 61) and allegedly favored "a program of enlarged government spending, financial reform, and redistributive taxation" that would increase consumer demand (Fraser 1989, 70). This group, so it is argued, found its political home in the Democratic Party.

To test these claims, we studied the campaign contributions from officers and directors of 53 top companies in seven mass-consumption industries: chain stores, mail order companies, department stores, household electrical equipment, food and beverages, brewing, and tobacco. The number of companies in each sector, along with the total number of officers and directors and the

TABLE 1. Campaign Contributions of \$100 or More of All Directors of the Leading Mass-Consumption Industries in 1936

Industry	Percentage of Contributors	Total Number of Directors
Chain stores (5)	20%	59
Mail order (2)	8	39
Department stores (10)	18	159
Household electrical equipment (6)	14	90
Food and beverages (10)	28	223
Brewing (10)	25	141
Tobacco (10)	20	130
TOTAL (53)	22	841

Note: The number of companies in each industry is given in parentheses after the name of the industry.

number and percentage contributing to either party, are presented in Table 1.

When we compared the party preferences of the 187 donors in mass-consumption industries to our baseline *Poor's* group, we found a small but statistically significant preference of nearly ten percentage points for the Democrats on the part of these donors, 26% versus 17%, ($\chi^2 = 8.01$; $p = 0.0005$; Pearson $r = 0.09$). If we next ask what factors are involved in this apparent support for the mass-consumption thesis, we find two. First, those who are top officers in the company as well as members of the board of directors, henceforth called "inside directors," are more likely to support Democrats than Republicans, 34% versus 18% ($\chi^2 = 5.59$; $p = 0.02$; Pearson $r = 0.16$). Second, those who tended to favor the Democrats by more than the baseline figure of 17% are concentrated in three of the seven mass-consumption sectors: brewing, department stores, and tobacco. These results are displayed in Table 2.

Because brewing, department stores, and the tobacco industry had higher percentages of Democratic contributors than our baseline *Poor's* sample, we subjected these three industries to further analysis. In all three cases, variables other than industrial sector seem to account for the higher proportion of Democratic contributors. In the case of the brewing industry, for which we analyzed all twenty-eight U.S. companies listed in *Poor's*, we think a unique factor is producing the atypical preference: The Democrats lifted Prohibition in 1933 and revived the industry. As Overacker (1937, 487) argues, "liquor paid its debt of gratitude to the Democratic Party" for this great favor through contributions to Roosevelt in 1936. This explanation fits with the fact that 87% of the inside directors for these companies gave to Roosevelt, whereas only 11% of outside directors did so, as shown in Table 2.

TABLE 2. Percentage of Democratic Party Contributors among Donating Directors in Mass-Consumption Industries, by Type of Director

Industry	Percentage of Directors	Inside Directors	Outside Directors
Brewing	46% (50)	87% (23)	11% (27)
Department stores	45 (29)	73 (11)	28 (18)
Tobacco	42 (26)	36 (14)	50 (12)
Food and beverages	14 (63)	19 (37)	8 (26)
Mail order	13 (8)	0 (2)	17 (6)
Household electrical equipment	8 (13)	0 (7)	17 (6)
Chain stores	0 (12)	0 (10)	0 (2)
TOTAL	29% (201)	38% (104)	19% (97)

Note: The number of individuals in each group is given in parentheses. Thus, 46% of the 50 directors in the brewing industry contributed to the Democrats.

TABLE 3. 1936 Party Contributions of Department Store Directors, by Religion

	Jewish	Non-Jewish
Democrats	57%	18%
Republicans	43	82
TOTAL	100%	100%
Number	(14)	(11)

Note: $\chi^2 = 3.90$, p = 0.05, Pearson $r = 0.39$.

For the department store sector, the data in Table 3 indicate that the higher Democratic percentage is largely explained by religious background. There were more Jewish than non-Jewish contributors in this sector, and Jews tended to give to the Democrats, whereas non-Jews gave as strongly to the Republicans as in our baseline *Poor's* sample.

When we look in more detail at the tobacco industry, we can see another factor operating: region. This can be determined because five of the tobacco companies in our sample were based in the South and five in the North. We began with a stratified sample of five southern companies (according to company balance sheets in 1936) and matched them with five northern companies equivalent in size, product, and number of employees. There were more southern than northern tobacco executives who contributed. Most of the southerners gave to the Democrats, and most of the northerners gave to the Republicans, as can be seen in Table 4.

Alerted by these findings concerning the potential importance of religious and regional variables in explaining Democratic contributions, we did a background study of all inside directors in all mass-consumption industries who made campaign donations in 1936. We found that twenty out of the thirty-one Democratic

TABLE 4. 1936 Party Contributions of Tobacco Industry Directors, by Region

	South	North
Democrats	80%	9%
Republicans	20	91
TOTAL	100%	100%
Number	(15)	(11)

Note: $\chi^2 = 12.76$; p = .0004; $r = .71$.

TABLE 5. Campaign Contributions of $100 or More by Officers and Directors of Companies Considered Part of the "Keynesian Elite"

Company	Percentage of Contributors	Number Contributing to Republicans	Number Contributing to Democrats	Number of Officers/Directors
Goldman, Sachs	50%	2	1	6
Lehman Brothers	44	3	1	9
Bank of America	5	1	0	22
Bowery Savings Bank	29	7	0	24
Curtis Publishing	8	0	1	13
R.H. Macy	45	2	3	11
Federated Department Stores	56	2	4	11
City Stores	23	0	3	13
Lit. Brothers	14	0	2	14
F.W. Woolworth	11	3	0	26
Marshall Field	23	3	1	17
Dennison	0	0	0	9
Endicott Johnson	0	0	0	11
United Fruit	25	4	2	24
American Tobacco	12	1	1	17
IBM	22	3	1	18
General Foods	38	7	3	26
Eastman Kodak	0	0	0	24
Chrysler Corp.	50	8	1	18
TOTAL	22	46 (66%)	24 (34%)	313

donors (64%) were either southern (ten), Jewish (nine), or Catholic (one), as compared to only seven of sixty-two Republican donors (11%). This translates to a chi square of 28.42 ($p = 0.0000$) and Pearson $r = 0.53$ between these three "outsider" variables and Democratic preference. When we add the fact that nine of the remaining eleven inside directors who were Democratic contributors were in the brewing industry, we can see that region, religion, and the lifting of Prohibition account for almost all the Democratic donations among the inside directors.

In addition to identifying specific industries said to prefer the Democrats, the mass-consumption theorists also name specific firms thought to demonstrate their thesis. Therefore, we compared the officers and directors of nineteen companies mentioned by Friedlander (1987) with the campaign finance data for 1936. As Table 5 shows, the great majority from this "Keynesian elite" (66%) who contributed to the 1936 presidential campaign favored the Republican Party. Since this still leaves 34% who donated to the Democratic Party, as compared to 17% in our baseline sample ($\chi^2 = 12.51$, $p = 0.0004$), we then studied the religious background of these individuals in standard biographical sources.

As might be expected from our earlier findings on the department store sector, the preference for the Democrats among donors in the "Keynesian elite" is best understood in terms of religious affiliation. For the sixty members of the Keynesian elite on whom we found religious information, 67% of eighteen Jewish donors gave to the Democrats, whereas 80% of forty-two non-Jewish donors gave to the Republicans ($\chi^2 = 14.56$; $p = 0.0001$; Pearson $r = 0.47$). Such large religious differences would not exist if business considerations were determining the party preference of business leaders in the mass-consumption industries.

INDUSTRIAL SECTORS AND PARTY ORIENTATION

The second revisionist claim concerning differences in business leaders' party preferences in 1936 has been made by Thomas Ferguson (1984, 1995), whose general "investment theory of party competition" proposes a systematic relationship between the economic policy interests of business leaders in specific sectors and their party preferences. In the case of the New Deal, he claims that firms and industries can be located along two primary axes: "labor sensitivity" and "international market orientation" (Ferguson 1984, 54). Labor sensitivity refers to the price business will pay for labor compliance, while international market orientation is concerned with the competitiveness of firms in the world economy.

Capital-intensive firms are said to be less labor sensitive, so business leaders in such firms are more willing to support a party making concessions to workers and unions. Internationally oriented firms tend to favor open markets, so business leaders in such firms are eager to support an internationalist and nonprotectionist party. If firms cluster in one of the four quadrants created by the capital-intensive and internationalist dimensions—for example, all are protectionist and labor intensive—then little political conflict will result. But from the 1920s to 1970s, according to Ferguson, firms tended to concentrate in two diagonally opposite quadrants, with labor-intensive, nationalist firms opposed by capital-intensive, internationalist ones. Ferguson argues that this produced a relatively stable party system in which there was a contest between the nationalist, labor-intensive firms supporting the Republicans and the internationalist, capital-intensive firms supporting the Democrats (Ferguson 1984, 48–61). As one type of evidence for his claims about the New Deal, he uses campaign donations.

TABLE 6. Campaign Contributions of $100 or More by Directors in Five Leading Industries

Industry	Percentage of Contributors	Total Number of Directors
Textiles (10)	11%	135
Steel (10)	41	201
Oil (10)	25	117
Commercial banks (10)	19	800
Investment banks (14)	36	170
TOTAL (60)	24%	1,423

Note: The number of companies in each industry is given in parentheses after the name of the industry.

In the original application of his theory to the 1936 election, Ferguson (1984, 50) argued that tobacco and oil were the two most capital-intensive industries and that textiles and steel were the most labor intensive. More recently, he has included the food and beverage industry in the capital-intensive category (Ferguson 1995, 220). In his original formulation, Ferguson also claimed that "non-Morgan" investment banks and large commercial banks were part of the capital-intensive, internationalist bloc. In addition, he has restricted this assertion to the ten largest commercial banks and the fifteen largest investment banking firms, minus Morgan Stanley (Ferguson 1995, 223).

In order to test these claims, we studied the donations of all inside and outside directors of (1) the ten largest steel companies in 1936; (2) five northern and five southern textile companies that were matched for company size and product type; (3) the ten largest oil companies; (4) all directors, vice-presidents, and second vice-presidents of the sixteen commercial banks with more than $500 million in 1936; and (5) all partners in the fourteen largest investment houses (minus Morgan Stanley). The total number of companies and directors for each sector, along with the percentage who made donations to either party, are presented in Table 6.

We then analyzed our data in terms of the Democratic support from all directors, inside directors, and outside directors for each sector in the same way we did for the mass-consumption industries. The results are presented in Table 7, which includes the findings for the tobacco and food and beverage industries presented earlier because they are relevant to Ferguson's claims. As can be seen, the only industrial sector with a higher than expected Democratic percentage is tobacco, but as we demonstrated earlier, this differential preference is due to those inside and outside directors who are affiliated with companies located in the South. The inside and outside directors of northern tobacco companies are overwhelmingly Republican in their preferences (91%).

Despite the general lack of support in Table 7 for Ferguson's claims, several comments and further analyses are necessary to deal with issues that are not apparent from the table. First, Ferguson (1995, 221) has claimed that he found the food and beverage sector to exceed his baseline Democratic preference figure. This finding is due to the fact that he included in this sector the brewing industry, but we have already shown that the greater preference for the Democrats on the part of its inside directors in 1936 was most likely due to the lifting of Prohibition. The dramatic difference between the brewing industry and all other food and beverage companies in the percentage contributing to the Democrats (46% versus 14%) is strong evidence for the uniqueness of the brewing industry. In fact, without the brewers, the food and beverage industry is under our *Poor's* baseline sample.

TABLE 7. Contributions to the Democratic Party among Donating Directors, by Type of Director and Industry

Industry	Percentage of Directors		Inside Directors		Outside Directors	
Tobacco	42%	(26)	36%	(14)	50%	(12)
Oil	21	(29)	28	(18)	9	(11)
Textiles	20	(15)	25	(8)	14	(7)
Commercial banks	18	(150)	9	(67)	25	(83)
Food and beverages	14	(63)	19	(37)	8	(26)
Investment banks[a]	10	(62)	—		—	
Steel	5	(83)	0	(47)	11	(36)

Note: The total number of individuals in each group is given in parentheses. Thus, 42% of the 26 directors in the tobacco industry contributed to the Democrats. Industries presented in the table are those used by Ferguson as key examples of the capital-intensive/internationalist and labor-intensive/nationalist sectors of business.

[a] There is no distinction between inside and outside directors in this industry since all are considered "partners."

Although the campaign donations of the directors of the large international oil companies originally emphasized by Ferguson (1984, 64–7, 80–4) provide no evidence for his thesis, it is necessary to analyze the industry as a whole because he has more recently stressed (1995, 214) that newer and smaller oil companies, such as those in eastern Texas, must be taken into account. To test this claim, we looked at all ninety-one American oil companies listed in *Poor's* for 1936. There was a difference of only seven percentage points between the directors from these companies who donated and our baseline *Poor's* sample, 24% versus 17%, and the chi square did not reach statistical significance at the 0.05 level. Nonetheless, as displayed in Table 8, there is a correlation of 0.17 between inside directors and Democratic donations within the industry, which is significant at the 0.04 level.

We then divided the inside directors in terms of northern companies and those based in Texas and Oklahoma, which are traditionally considered southern states, even though Oklahoma was not part of the

TABLE 8. 1936 Party Contributions of Oil Company Directors, by Type of Director

	Inside Directors	Outside Directors
Democrats	32%	15%
Republicans	68	85
TOTAL	100%	100%
Number	(57)	(53)

Note: $\chi^2 = 4.13$; $p = 0.04$; $r = 0.17$.

TABLE 9. 1936 Campaign Contributions of $100 or More from Partners of the Major Investment Banks

Company	Percent of Contributors	Number Contributing to		Total Number of Partners
		Republicans	Democrats	
First Boston	4%	1	0	24
Dillon Read	33	2	1	9
Kuhn, Loeb	50	5	0	10
Blyth & Co.	0	0	0	14
Edward B. Smith	59	10	0	17
Charles D. Barney	15	2	1	20
Lehman Brothers	44	3	1	9
Brown Brothers, Harriman	44	3	1	9
Kidder, Peabody	80	4	0	5
Goldman, Sachs	50	2	1	6
White, Weld	38	5	0	13
Eastman, Dillon	45	5	0	11
Drexel & Co.	55	9	1	18
Lazard Freres	100	5	0	5
TOTAL	36%	56 (90%)	6 (10%)	170

Confederacy. We found that the Texas/Oklahoma companies were contributing a higher percentage to the Democrats, 50% versus 24% ($\chi^2 = 3.49$; $p = 0.06$). When we subtracted the sixteen inside directors from these southern companies and recalculated the Democratic percentage; it was 19%, only two percentage points above our baseline *Poor's* sample. Thus, our analysis shows that any tendency toward differential support for the Democrats in the oil industry is primarily due to inside directors in companies based in Texas and Oklahoma, as Overacker (1937, 487) also concluded. Once again, region is a key factor in understanding the party preferences of business leaders who made campaign donations.

Despite the fact that the directors of the sixteen largest commercial banks as a whole did not differ from our *Poor's* baseline sample ($\chi^2 = 0.13$, $p = 0.71$), there was nonetheless an unexpected preference for the Democrats on the part of 25% of the outside directors as compared to only 9% of the inside directors and nondirector vice-presidents ($\chi^2 = 6.71$; $p = 0.01$; Pearson $r = 0.16$). When we studied the background of the twenty-one outside directors who gave to the Democrats, eight were either southern, Catholic, or Jewish. In addition, we found that three of them (Vincent Astor, Cornelius Vanderbilt, and Cornelius Vanderbilt Whitney) were highly unusual in that they were members of extremely wealthy families but not active officers in any banks, investment firms, or corporations. Furthermore, these three men also sat on an average of thirteen other corporate boards, which is far more than the typical director of the era. Thus, these three join with southerners, Catholics, Jews, and brewers in accounting for most of the small number of inside and outside directors in each business sector who supported the Democrats financially in 1936.

Finally, as shown in Table 9, it is noteworthy that only six of sixty-two (10%) partners in the fourteen top investment banks favored the Democrats, and no firm had a majority contributing to the Democratic Party. The four investment bankers named by Ferguson (1984, 91) as examples to support this theory are the only ones from their firms who donated to the Democrats.

In summary, our analysis of Ferguson's claims about business preferences in the 1936 presidential election finds that none of the five capital-intensive or internationalist sectors were more likely to support the Democrats for the reasons he gives. Instead, region and the lifting of Prohibition explain any differential preferences. Given the lack of evidence for Ferguson's theory or the claims of the mass-consumption theorists, it is useful to make further analyses of campaign contributions by business leaders in 1936. We present findings from such analyses in the following section.

PARTY PREFERENCES OF BUSINESS LEADERS DURING THE NEW DEAL

In order to obtain a new general picture of the party preferences of business leaders in 1936, we did three further analyses. First, we drew a random subsample of 1,584 donors from the sample of 3,098 donors used earlier. This smaller sample was necessary due to time constraints but was large enough for sound statistical analysis. Second, we studied every contributor in the southern states (the eleven states of the Confederacy plus Kentucky and Oklahoma, a total of 1,121 donors) because the region was a major stronghold for the Democrats at that time; 37.8% of Democratic contributions in 1936 came from the South (Overacker 1937, 495). Third, we studied every donor who listed New York City or one of its suburbs as a business or home address, a total of 1,409 donors, because that area was the single largest urban source of campaign contributions for both parties in the 1936 election. New York was also the home of more large corporations than any other city, which maximizes the possibility of finding major business leaders who were Democratic contributors. Based upon Overacker's findings (1937, 496), we estimate that 16% of Democratic contributions over $100 came from New York in 1936.

For these analyses we used two indicators of business standing. First, we determined which donors were listed as executives or directors in the 1936 edition of *Poor's*. Second, as an indicator of whether a person was a "top" business leader, we determined which of the donors listed in *Poor's* were also affiliated with one of the largest 200 nonfinancial corporations, 30 commercial banks, 20 investment houses, or 20 insurance companies on a list of the top 270 business firms for 1936. This list was compiled by Allen (1991) based upon data collected by the Temporary National Economic Committee in the late 1930s. Thus, we can determine whether a person is (1) not in *Poor's*, which implies the individual is not in business or is with only a very small firm; (2) affiliated with a firm listed in *Poor's*; or (3) with one of the largest 270 firms in the United States in 1936.

In our national subsample, where 65% of all donors gave to the Republicans, we found that Republican donors were far more likely than Democratic donors to be listed in *Poor's* (43% to 16%). This finding can be understood as a correlation of 0.27 between a Republican preference and the business community; the chi square of 117.25 is significant at the 0.0000 level. In terms of those donors listed in *Poor's* who were also connected with at least one of the 270 largest firms, we once again found a significant positive relationship with Republican donations ($\chi^2 = 4.01$; $p = 0.05$), but the relationship was not as strong ($r = 0.09$).

In the South, where 87% of the 1,121 donors in those states gave to the Democrats and where the Republicans were still demonized as the party that freed the slaves, we expected to find that a greater percentage of Democratic than Republican donors would be listed in *Poor's* and that a higher percentage of the Democratic donors listed in *Poor's* also would be connected with a top-270 firm. Neither turned out to be the case. Instead, the correlation between being a Republican donor and being listed in *Poor's* was a positive one (0.16). Of the Republican donors, 27% were listed in *Poor's*, but only 11% of the Democratic donors ($\chi^2 = 27.77$; $p = 0.0000$). This finding once again supports the idea that the Republican contributors were more connected to major businesses than were the Democratic donors. In terms of connections to top-270 firms, however, there was no difference between the two parties in the South. Only 8% of Republican and 7% of Democratic donors were connected to a top-270 firm, reflecting the generally smaller size of most businesses in that region.

In New York, where 68% of the 1,409 donors in that city gave to the Republicans, a figure very similar to that for the national subsample, the relationship between Republican donations and being listed in *Poor's* was once again statistically significant ($\chi^2 = 18.2$, $p = 0.0000$), as it was for the national subsample and the entire population of southern donors. It is interesting, however, that the correlation is lower ($r = 0.12$) than for either the national subsample or the South ($r = 0.27$ and 0.16, respectively). This is because a higher percentage of Democratic donors are listed in *Poor's* (36% versus 48% for the Republicans). Relatively speaking, then, New York was more favorable for the Democrats than the South or the nation as a whole when it came to contributions in 1936 from business donors from large firms.

The notion of New York as a relative stronghold for the Democrats in terms of major business connections gains further support from our analysis of campaign donors' affiliations with one of the 270 largest U.S. corporations. For Republican donors, the percentage listed in *Poor's* who were also connected with a top-270 firm is 31%, which is eight percentage points higher than the party's figure in the national subsample. For the Democratic donors, however, the figure increases to 29%, a little more than double their top-270 percentage in the overall subsample and essentially indistinguishable from the Republicans (χ^2 is not significant).

There were 47 Democratic and 142 Republican donors from New York who were connected with top-270 firms. They are by and large the partners, officers, and directors in the investment banks, commercial banks, retail stores, and manufacturing firms that we analyzed in earlier sections. Most of the Democratic donors were a minority of one or two among officers or directors on boards, the majority of whom gave to the Republicans, and they are not overrepresented in mass-consumption or capital-intensive companies. Therefore, they provide no support for the claims of either Fraser or Ferguson. The few companies that had three or more Democratic donors from New York were in brewing and retailing, which we discussed earlier.

DISCUSSION

None of the findings in the previous sections provide any support for the analyses put forth by Ferguson and Fraser. It is region, religion, and the lifting of Prohibition, not the policy needs of any given business sector, that best explain any greater tendency to contribute to the Democrats. Yet, our findings on donations by New York–based directors of *Poor's* and top-270 firms may help explain some of the misunderstandings in the work of Ferguson and Fraser, who overstate the preference of top business leaders for the Democrats for two main reasons.

First, their focus is primarily on New York, where there was a higher percentage of Democratic donors from *Poor's* and top-270 firms than in the country as a whole or the South. Second, because they often argue from examples of important Democratic donors in the business community, they are overly focused on the forty-seven Democratic contributors in New York who were connected with a top-270 firm. Of the Democratic supporters listed as part of the Keynesian elite by Friedlander (1987), eight of nineteen (42%) were from this atypical forty-seven. Of the examples of Democrat supporters listed by Ferguson (1984) for the capital-intensive sectors, twenty of forty-seven (43%) are on our New York list of forty-seven Democratic donors connected with a top-270 firm.

Although our findings do not support the basic claims made by the mass-consumption theorists or Ferguson, they are highly compatible with other findings on the two parties. They support Overacker's (1937, 485–9, 497–8) overlooked conclusion that the Democrats were supported in 1936 by southern businessmen and smaller businessmen outside the South as well as lawyers, officeholders, and organized labor. They also support Domhoff's (1990, 225–55) more general historical analysis of the main backers of the Democratic Party.

Second, these findings are in accord with those of Allen (1991) on the 1936 campaign donations by wealthy families and by the inner circle of top corporate executives in that era. He estimates that 80% of the major capitalists who contributed to the 1936 presidential campaign gave to the Republicans, which is very similar to our baseline *Poor's* sample. Moreover, as in our analysis, Allen concludes that "socially marginal 'outgroups' like Southerners and Jews were much more supportive of the Democratic Party" (1991, 687).

Third, our findings on the religious differences in the party preferences of business leaders in the department store sector are in keeping with those of Webber (1990, 135–40) on New York City for 1936, of Alexander (1971b) for the donations of New York businessmen in 1948 and 1952, of national surveys of the general population for 1952, 1956, and 1960 (Heard 1962, 424), and of Domhoff (1990, 225–55) for the 1968 and 1984 elections. These studies suggest Jewish business leaders favor the Democratic Party, although not nearly as strongly as northern Protestant business leaders favor the Republicans. Some observers (e.g., Cohen 1989, Isaacs 1974) estimate that members of the Jewish community have provided the Democrats outside the South with as much as half their financial support since the 1960s.

Fourth, our results are compatible with studies showing the lack of differences among business sectors in contributions to congressional campaigns in the 1980s (Clawson, Neustadtl, and Bearden 1986; Clawson and Su 1990; Neustadtl, Scott, and Clawson 1991; Su, Neustadtl, and Clawson 1995). These studies have demonstrated that there is a high degree of class-based unity in corporate political contributions, with business giving strong support to conservative candidates in both the North and South.

Fifth, our findings are consistent with studies of voting patterns showing that southern and Jewish voters differentially supported the Democrats in the 1936 presidential elections (Allswang 1978, Campbell et al. 1960, Ladd and Hadley 1978). Religion and region were to remain important correlates of voting patterns, especially for Catholics, Jews, and white southerners, who continued to be loyal Democratic supporters for many years after the New Deal.

CONCLUSION

While the claims made by Fraser and Ferguson give the impression that the patterns of campaign contributions in 1936 are at odds with other findings about the two parties, and in opposition to the general conclusion that big business as a whole opposed Roosevelt, our findings suggest that campaign contributions are best understood by what social scientists have learned about the two parties from their voting studies. As Overacker (1945, 915) concluded on the basis of her research into campaign donations for the 1936, 1940, and 1944 presidential elections: "The further one goes into the financial backing of the two parties the more convincing is the evidence that they receive their support from very different groups, with what might even be called distinct characteristics."

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The European Parliament as a Conditional Agenda Setter: What Are the Conditions? A Critique of Tsebelis (1994)

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Tsebelis (1994) argues in the American Political Science Review *that the European Parliament has important power due to its right as a conditional agenda setter. I claim that Tsebelis' argument is based either on an incomplete analysis or on inaccurately specified decision rules. An accurate modeling of the cooperation procedure as stated in Article 189c of the Treaty of the European Community and as applied in practice changes the results considerably. Based on such a model, I provide an explanation of why the European Parliament sometimes can make successful amendments.*

In an influential paper on the cooperation procedure in the European Union (EU) that focuses, among others, on the legislative decision-making rule with which the internal market program was implemented, George Tsebelis (1994) argues that the European Parliament (EP) has an important effect on EU decisions due to its power as a conditional agenda setter. He claims that the EP can place items on the legislative agenda which would not otherwise be considered and that such amendments can sometimes result in more integration than otherwise would be chosen. Tsebelis predicts that, within the cooperation procedure, the influence of the EP is likely to increase in the future due to the curvilinear property, according to which the agenda-setting power of the EP increases if the status quo approaches the Pareto set of the Council of Ministers; that power reaches a maximum at some point and declines if the status quo moves closer to the EP's ideal position. Most important, Tsebelis argues that, in contrast to previous research, his model is capable of explaining the observation that the Commission of the European Union often accepts amendments proposed by the EP.

I would like to raise three points. First, I argue that the decision rules modeled by Tsebelis do not accurately reflect the cooperation procedure as stated in Article 189c of the Treaty of the European Community and as applied in practice. Second, a complete and accurate modeling of the cooperation procedure changes Tsebelis' results substantially. Third, I provide an explanation consistent with these decision rules to show why the EP sometimes can successfully amend the common position chosen by the Council. Such an explanation, however, requires a careful analysis of whether restrictions that the European Parliament faces have changed during a particular decision process, such that the EP can take advantage by proposing an amendment.

TSEBELIS' MODEL

In accordance with Tsebelis, I assume that the Council is composed of seven members with Euclidian preferences and that the qualified majority rule requires five votes. To simplify the exposition, I reduce Tsebelis' two-dimensional model to a choice along one dimension (e.g., the degree of market integration). This simpler model allows me to represent his most important claims regarding the influence of the EP and the curvilinear property.[1] Figure 1 depicts the ideal preference points of the seven Council members (*1* to *7*), the status quo (*SQ*), the ideal point of the EP, and two different ideal points of the Commission (C_1, C_2). The preference distribution (with C_1) reflects the standard argument also followed by Tsebelis that the Commission and the EP prefer more integration than do the members of the Council. Furthermore, I assume that all players have complete and perfect information and that none of the players prefers its decision to be overturned, which rules out decisions to be reversed by other players.

The procedure in the Tsebelis model is the following.

1. "*The Council can unanimously adopt any proposal inside the area indicated by the unanimity [win] set of the status quo, or* U(SQ)" (p. 132, emphasis added). In Figure 1, this set includes all points between the status quo and the point which keeps the least integrationist member (*1*) indifferent to the status quo. To get the smallest possible effect of the EP, Tsebelis assumes that the Council selects the point inside $U(SQ)$ most preferred by the EP. In Figure 1, this is point z. Notice that if the status quo is farther to the left, $U(SQ)$ increases. In contrast, if the status quo is inside the Pareto set of the Council—the interval between *1* and *7*—$U(SQ)$ is empty.

2. In the second stage of Tsebelis' model, the EP can make a proposal that is approved if preferred to z by the Commission and by a qualified majority in the Council. The qualified majority win set of z, $Q(z)$, represents the latter constraint, depicting the set of points preferred to z by the decisive Council member (*3*). If the Commission's ideal point is at C_1, then the binding constraint is $Q(z)$, such that the EP can select x, which becomes the new status quo.

One major result of this decision structure is the curvilinear property of the relation between the location

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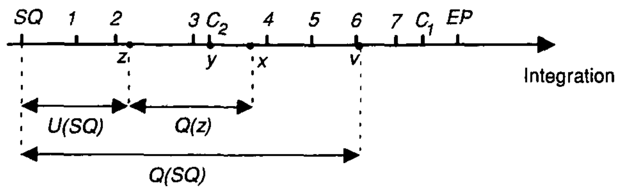
[1] I agree with Tsebelis about the fundamental differences between one- and two-dimensional models (see his footnote 5), but the curvilinear property and the influence of the EP can be illustrated by a one-dimensional representation. The only effect that thereby disappears is the possibility of selecting among *different* coalitions inside the Council (see Figure 4 in Tsebelis). It is left to the judgment of the reader as to which representation makes the argument more comprehensible.

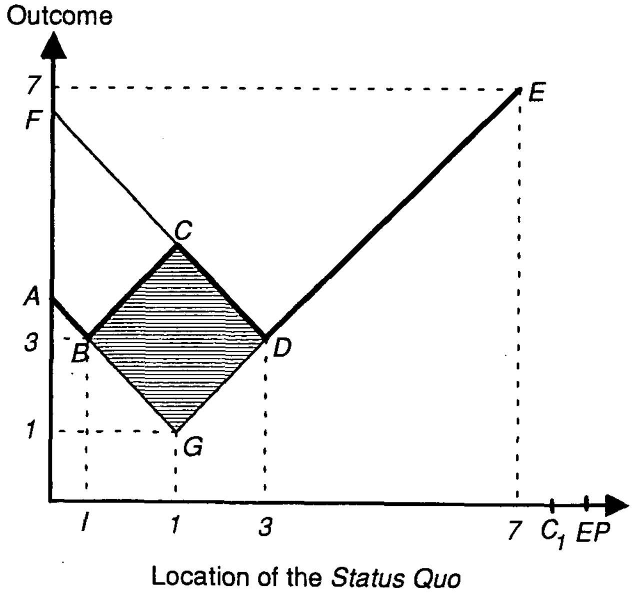
FIGURE 1. Preference Configuration and Win Sets



U(SQ): Unanimity set of the *status quo*
Q(z): Qualified majority win set of *z*
Q(SQ): Qualified majority win set of the *status quo*

of the status quo and the agenda power of the EP. Figure 2 shows the equilibrium outcome as a function of the location of the status quo for the same preference configuration as in Figure 1. The bold line (*ABCDE*) represents the outcome with Tsebelis' procedure. The status quo cannot be changed if *SQ* is at or to the right of 3 (line *DE*). If $1 \leq SQ < 3$, no unanimous change is possible, and Tsebelis argues that "*consequently, the status quo can be modified only through a parliamentary proposal*" (p. 133, emphasis added). Hence, the EP can choose its most preferred point inside the qualified majority win set of *SQ*, *Q*(*SQ*), and thereby realize an outcome on the line *CD*. Situations analogous to Figure 1 occur if *SQ* is between *1* and *I*. *I* is the point that makes Council member 1 indifferent to the policy position at *3*. Finally, if $SQ < I$, the Council chooses *z* unanimously at the maximum of *U*(*SQ*). In this case, the common position (*z*) is to the right of the ideal point of member 3, who is decisive under the qualified majority rule. Therefore, *Q*(*z*) is empty, and the EP cannot change the Council's decision.

FIGURE 2. Equilibrium Outcome and the Location of the Status Quo



The line *AGE* reflects the outcome of the first stage in Tsebelis' procedure: The Council acts unanimously and always chooses the highest degree of integration inside *U*(*SQ*). The shaded area represents the influence of the EP, according to Tsebelis' curvilinear property: If the status quo is far outside the Pareto set, the EP has no effect. If the status quo approaches the Pareto set, the influence of the EP increases, reaching its maximum if $SQ = 1$, and then declines again. This result occurs because Tsebelis supposes the following two-stage procedure: First, the Council decides unanimously without any agenda rights by the EP or the Commission. Second, the EP can make a proposal which the Council considers under the qualified majority rule. Notice that this result differs from the usual agenda-setting effects, as analyzed by Romer and Rosenthal (1978). Their model has only one stage, as is the case if the EP or the Commission can make a proposal that must be approved by a qualified majority in the Council. Thereby, the agenda setter can choose his or her most preferred policy inside the qualified majority win set of *SQ*, *Q*(*SQ*) (policy *v* in Figure 1). The line *FDE* in Figure 2 depicts the equilibrium outcome with this rule: The farther to the left the status quo is from the position of the final decision maker (here, the decisive member in the Council), the greater is the influence of the agenda setter.

In the preceding discussion, the EP and the Commission have similar preferences. To ascribe conditional agenda power to the EP, however, as Tsebelis does, it is necessary to show that the agenda-setting right of the EP is stronger than that of the Commission. Hence, a conflict of interest between these two players has to be considered. Conflicting interests exist if the two players prefer different *feasible* policies, both of which are supported by the Council. Tsebelis analyzes a conflict in his Figure 6 for a two-dimensional choice. His argument can again be represented for a one-dimensional choice. If the ideal point of the Commission is at C_2 in Figure 1, such that the Commission still prefers *x* to *z* and, as Tsebelis assumes, "the Commission can only accept or reject the EP's proposal" (p. 138), the Commission will accept *x*.

All these results depend crucially on the assumption that the Commission cannot or does not make the first proposal in the cooperation procedure, but this is an inaccurate or incomplete description of the procedure. It is inaccurate if Tsebelis' analysis is focused on the whole cooperation procedure, which he claims in his introduction: "*I examine the logic and the outcomes of decision making among three institutional actors generated by the cooperation procedure*" (p. 128, emphasis added). It is inaccurate in this case because the Commission always makes the first proposal, as I will discuss in the next section. If his analysis is limited to the second reading (see his remark on p. 131), then it is *incomplete* because he does not consider that the result of the second reading influences the strategic choices of the players in the first reading. Rather, his analysis is implicitly based on an asymmetric assumption: While the EP acts strategically in the second reading, the Commission does not use its agenda rights effectively in the first stage. In either interpretation, Tsebelis does not provide a convincing model of the cooperation procedure. In the following section, I model the agenda rights according to the treaty, incorporating both stages of the cooperation procedure, and present results fundamentally different from Tsebelis.

THE COOPERATION PROCEDURE RECONSIDERED

The cooperation procedure comprises five stages. (1) The Commission submits a proposal (y) to the Council. (2) After consulting the EP (first reading), the Council determines in its first reading the common position: It can approve y by a qualified majority or modify the Commission's proposal unanimously. (3) The EP can accept, modify, or reject the common position in its second reading. (4a) If the EP *accepts* the common position, the Council can adopt the common position by qualified majority rule in its second reading; otherwise the status quo prevails. (4b) If the EP *rejects* the common position, the Council is required to approve the common position unanimously; otherwise, the status quo prevails. (4c) If the EP *amends* the common position, the Commission decides whether to approve the amendment. (5a) If the Commission *approves* the amendment of the EP, the Council can accept the amended proposal by qualified majority or must pass it unanimously if modified. (5b) If the Commission *rejects* the amendment of the EP, the Council can approve the unamended common position by qualified majority rule or accept the proposal of the EP by unanimity.

This decision process remains undisputed, but Tsebelis claims that, in practice, the decision makers deviate from these rules in important aspects. First, he asserts that "*all three institutional actors* [Commission, Council, EP] *can in fact place items on the legislative agenda*" (p. 131, emphasis added). Thereby, Tsebelis claims that the EP has unrestricted proposal power. This stands in contrast with the legal literature, which argues that the Commission has substantial agenda control. Although the Commission agreed in 1982 to take up any parliamentary proposal *to which it does not have any major objections* (see Jacobs and Corbett 1990, 181), and although a majority of the EP members can request the Commission to submit a proposal since the Maastricht treaty (Article 138b), the Commission is free to decide *when* to make the proposal and *what* to include in it (see von der Groeben et al. 1991, 4307, and Lenz and Borchardt 1994, 1018 and 1059). Some legal scholars (Kapteyn and Verloren van Themaat 1989, 252) even argue that such requests are nonbinding. In any case, the Commission does have broad agenda control and is not bound by the suggestions of the EP.

Second, Tsebelis' model implies that the Council unanimously chooses a proposal on its own in the first stage. Such an interpretation underestimates the influence of the Commission, which always makes the proposal (independent of whether the issue was suggested by the EP or by the Council); the Council can modify the proposal only unanimously but approve it by qualified majority rule.

My interpretation of the cooperation procedure leads to three results that differ substantially from Tsebelis' conclusions.

Result 1

Given the preference configuration in Figure 1, actual policy shifts more toward integration than Tsebelis' model predicts. Referring to Figure 1, x is the maximum integration that can be achieved according to Tsebelis' model. Due to the Commission's agenda rights in the first stage, however, the Commission can choose its most preferred policy in the qualified majority win set of the status quo, $Q(SQ)$, that is also in the Pareto set (otherwise, the Council can modify the proposal). For example, a Commission with an ideal point at C_1 chooses v, and the Council members cannot reverse policy to z. This would require consent. Members 6 and 7, however, would veto any move to the left of v. Furthermore, v is approved by a qualified majority because the decisive member 3 is better off with policy v than with the status quo. Referring to Figure 2, the equilibrium outcome is depicted by the line FDE, the outcome predicted by the Romer-Rosenthal model. Notice, first, that for $SQ < I$ the policy shifts more toward integration than in Tsebelis' model and, second, that the curvilinear property vanishes: The farther away the status quo from the Pareto set, the closer the realized policy is to the agenda setter's most preferred policy.[2]

Result 2

If there is no conflict between the Commission and the EP (because both prefer more integration than the decisive member in the Council), and if the positions of the players do not change during the decision process, then the EP never has the opportunity to change the outcome. This is the standard preference configuration used by Tsebelis to analyze the effect of the EP. In this situation, however, the EP never makes an amendment but remains inactive, because the Commission has already made the optimal proposal (v in Figure 1) in the first stage. The difference between Tsebelis' model and mine is most obvious if the Commission and the EP have conflicting interests.

Result 3

If the Commission and the EP have conflicting interests, the EP's right to suggest amendments has no effect when both players prefer a change in the same direction and when the EP's optimal policy is farther away from the status quo than that of the Commission.

Suppose that the ideal point of the Commission is at C_2 in Figure 1. In this case, Tsebelis' model implies that the EP can propose x successfully. Since he implicitly assumes that the Commission does not use its agenda rights effectively, the Council would unanimously choose z. Given z, the EP would propose x, which the Commission prefers to z. Yet, if the Commission behaves as strategically as the EP, the Commission would propose in the first stage its most preferred policy (y), which the Council would adopt as common position by a qualified majority. The EP prefers more integration, but such a policy change would not be supported by the Commission and would fail to receive the required unanimous support in the Council. Hence, the Commission's optimal policy (y) is realized, and the EP has no effect on the decision. Only if the EP's ideal point is closer to the status quo than that of the Commission and that of the decisive Council member can the EP force the Commission to propose a smaller policy change than it would do otherwise (see Steunenberg 1994). In this respect, the EP is a *conditional veto player*. Since the veto of the EP can be outvoted by a unanimous vote of the Council in the cooperation procedure, the EP can at best force the Commission to choose a point inside the unanimity win set, $U(SQ)$.[3]

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[2] This point has been independently developed by Crombez (1996).

Summary

A complete and accurate modeling of the cooperation procedure changes Tsebelis' results substantially and reestablishes the traditional claim that the EP is weak. In contrast to his results, my argument implies that the Commission, not the EP, is the decisive agenda setter. The power of the Commission does not correspond to the curvilinear property. Rather, its power increases the farther away the status quo is from the Pareto set. This implies that the agenda influence of the Commission is likely to decrease as the status quo approaches the position of the Council members—and does not increase as conjectured by Tsebelis (p. 139). In addition, the effect of a progressive EP is much more limited than Tsebelis argues; in most cases the EP can only beg the Commission and the Council to reconsider.

Finally, the influence of a prointegrationist EP is not modified in the codecision procedure (Article 189b) because amendments by the EP are treated similarly in both procedures. Also in the codecision procedure, amendments have to be approved by a qualified majority of the Council if the Commission supports them, otherwise by unanimity. The major difference between these two procedures is that the EP can be outvoted by a unanimous Council in the cooperation procedure, while the EP has an absolute veto power in the codecision procedure. That is, if the Council does not approve the amendments and the Conciliation Committee fails to reach a compromise, then the Council can confirm the common position "*to which it agreed before the conciliation procedure was initiated, possibly with amendments proposed by the European Parliament*" [Article 189b(6), emphasis added]. The EP can only threaten to veto the whole legislation and thereby maintain the status quo, which is of limited value for a prointegrationist EP.[4]

INTERPRETATION OF EMPIRICAL EVIDENCE

Tsebelis' research is motivated by empirical evidence that amendments by the EP are surprisingly often accepted by the Commission and supported by the Council. Summarizing data from 1987 to 1991, Tsebelis concludes that "*of every four EP amendments, two are accepted, one is rejected by the Commission, and one is rejected by the Council*" (p. 136, emphasis added). Some of these amendments demanded major policy changes, as in the case of automobile emission standards. If the EP is so weak, as I argue, then why does it have such a profound effect on EU policy choices in some cases?

While I regard the explanation offered by Tsebelis as unconvincing, he deserves credit for addressing this important issue in a rigorous manner. There is a more persuasive way to explain the empirical evidence, however, based on accurately specified decision rules: Once the Council has adopted the common position, the Commission can no longer change its proposal (Article 189a[2]) even if it wishes to do so. At this stage, only the EP can suggest amendments. Such proposals can be successful *if restrictions have changed* between the adoption of the common position in the Council and the final reading in the Council.

I can identify three kinds of potential changes in the restrictions that make a successful amendment by the EP feasible (see Moser forthcoming for a comprehensive discussion). First, consider a situation in which the decisive Council member is the binding restriction, that is, the Commission and the EP want more integration, but the Commission is constrained in its proposal at the first stage by the requirement to be supported by a qualified majority in the Council. Suppose that after the adoption of the common position in the first reading of the Council, the decisive member changes position toward more integration. Using Figure 1, this implies that the ideal point of member 3 moves to the right. Such a revision of a government's position can be due to a change of the party (or party coalition) in power or to altered public perception of an issue. Both the Commission and the EP would like to take advantage of this change, but only the EP can propose amendments to the common position after the first reading in the Council. Consequently, the EP makes an amendment which is supported by the Commission (and which the Commission could not make in the first stage) and by a qualified majority in the Council.

Second, consider a situation in which the Commission's ideal point is closer to the status quo than that of the decisive member in the Council. Hence, the Commission can realize a common position at its ideal point. If its preferences change (e.g., the responsible Commissioner is replaced), then any change in the Commission's position toward the ideal point of the EP can be exploited by the EP in its second reading. In Figure 1, suppose that the Commission's ideal point was at *2* and moves to *3*. With the common position at *2*, the EP can make a proposal to the right of *3* such that the Commission is marginally better off than with the common position at *2* and such that decisive Council member 3 supports it.

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[3] Of course, the EP can (and does) substantially delay a policy change already in the first reading because the Council can only approve a common position if the EP has given its opinion. There is no time limit for the EP to act, although it is doubtful whether the EP could openly block decision-taking in the conciliation procedure or in the first reading of the cooperation procedure (see Jacobs and Corbett 1990, 165).

[4] In one respect the influence of amendments by the EP may increase in the codecision procedure compared to the cooperation procedure. In the Conciliation Committee, the Council and the EP can agree on amendments that may be opposed by the Commission and approve them by qualified majority and absolute majority, respectively. Tsebelis (1995) comes to the different conclusion that the codecision procedure *reduces* the influence of the EP because he assumes that the Council is free to choose a policy if no agreement is reached in the Conciliation Committee. This assumption contradicts Article 189b(6).

Third, perceptions of what might happen if the status quo prevails can change. Quite often in EU politics, member states that advocate a major policy change threaten to enact it unilaterally if no satisfactory decision is forthcoming in the EU. The possibilities for unilateral action by the member states are defined by the treaty. In addition, a decision by the European Court of Justice can modify the perceived possibility of unilateral actions. If such a decision occurs in the course of decision making, the reversion policy may change such that the Commission is prepared to adopt a previously unacceptable amendment by the EP in order to avoid unilateral policies by some member states. Hubschmid and Moser (forthcoming) argue with respect to the well-known case of automobile emission standards that one factor inducing the Commission to accept the EP amendment was the change or clarification in the position of the court in the Danish bottle case, which involved whether member states can restrict the free movement of goods to protect the environment. After this decision, it became clear that members who sought strict environmental standards (including those on car emissions) were likely to be allowed to do so on their own. Since the Commission wanted to avoid an erosion of the common market for cars, it modified its position, withdrew the suit at the court, and accepted the stricter standards proposed by the EP.

CONCLUSION

By modeling the complete cooperation procedure accurately, I show that the EP has only limited powers and thereby disagree with Tsebelis. In particular, when the EP prefers a more substantial policy change than does the Commission, in most cases it can only petition the Commission to revise its opinion. If the EP were to prefer a smaller policy revision or a change in the opposite direction from the Commission, however, then the veto rights of the EP would become effective. In this respect, the cooperation procedure grants the EP *conditional veto rights*, depending on particular preference configurations. Furthermore, if in the future the status quo approaches the position of the members in the Council, then the influence of the agenda setter is likely to decrease, not increase, as argued by Tsebelis.

Yet, the EP's amendment rights can shift EU decisions closer to the EP's prointegration position if the views of the key players change after the adoption of the common position. Examples are shifts in the position of the decisive Council member or of the Commission, or modifications in the reversion point. Whether such changes in the restrictions can explain the large number of successful amendments by the EP is an open empirical question that requires careful analysis of individual cases.

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More on the European Parliament as a Conditional Agenda Setter: Response to Moser

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Moser analyzes the cooperation procedure using a model that assumes (1) one dimension and (2) complete information. I show that because of these two restrictive assumptions and his misunderstanding of the strategic implications of Article 189a(1) of the Maastricht Treaty (an article also present in the Single European Act and the Treaty of Rome), Moser's conclusions are mistaken. In particular, his predicted outcomes are incorrect, and his major institutional prediction (that the European Parliament plays no role in decision making except in the second round of the cooperation procedure) is contradicted by thousands of parliamentary amendments, the major part of which are accepted in the first round.

The article that Peter Moser criticizes (Tsebelis 1994) uses a multidimensional Euclidean model to investigate interactions among the Council of Ministers, the Commission of the European Union, and the European Parliament (EP) in the second round of the cooperation procedure. On the basis of the model, I drew several conclusions concerning this interaction and, more generally, decision making in Europe. Such conclusions concerned what I called (1) the curvilinear property, that in one or two dimensions when the status quo is far away from the Council or in the middle of the ideal points of the Council members, neither the Commission nor the EP has agenda setting power; (2) the "informational property," that the more secretive the deliberations of the Council, the less influence the agenda setter has; and (3) the "dimensionality of space property," according to which higher dimensionality confers more powers on the agenda setter. In addition, I concluded that the EP has conditional agenda-setting powers, and I speculated about the rate of integration when such powers are offered to prointegration actors like the EP and the Commission.

Moser presents a one-dimensional complete information model of European decision making and comes to three main results, two of which contradict my analysis. In this rejoinder I will show that his model is based on an incomplete understanding of the treaties and of my own article as well as a serious neglect of the facts surrounding the application of the cooperation procedure. Despite this, Moser makes some valid points, and I will try to sort them out from the rest of his argument. I start with a discussion of the three results of Moser's model and then argue that the reason his conclusions are incorrect is his limiting assumptions (complete information and one dimension). I then conclude.

MOSER'S RESULTS

My presentation follows the sequence in which Moser presents his results. I will demonstrate that our first and most important dispute (his Result 1) is based on his inaccurate understanding of the strategic implications of the treaties; the second (Result 2) is based on neglect of the facts; and the third (Result 3) is not a disagreement, since his point is one made throughout my article, including the summary. For reasons of economy of space I will refer to his results by number, and I will use his Figure 1 to detail our differences.

Result 1, or, What Happened to the Treaties?

I have claimed that in one- or two-dimensional policy space there is a curvilinear property to the EP's power. That is, if the status quo is very far from the ideal points of the members of the Council, or in the middle of them, the EP cannot make a winning proposal, whereas if the status quo is in intermediate position with respect to the Council, such a proposal may exist. Moser disputes this result and claims that the farther the status quo from the Council, the more power the agenda setter has. Let me add here what is implicit but not highlighted in my original article: If the policy space has more than two dimensions, then the farther away the status quo is from the members of the Council, the *less* power the agenda setter has. This statement will make my disagreement with Moser even more pronounced.

According to Moser, "Tsebelis' model implies that the Council unanimously chooses a proposal on its own in the first stage." He also claims that (in the configuration of forces depicted in his Figure 1) the Commission will exercise its agenda-setting power in the first round and make a proposal which is more prointegrationist than in my model, and that this proposal will be voted in the first round and cannot be upset in the second. Both claims are mistaken: I have never made the argument he attributes to me; moreover, an understanding of the strategic implications of the treaties shows that the agenda setter (whether the EP or the Commission in this case is irrelevant) is very unlikely to make Moser's proposal (although, as I will explain, under certain extreme conditions such a proposal *may* be possible).

My difference with Moser is located in Article 189a(1) of the Maastricht Treaty.[1] It specifies in part: "Where in pursuance of the Treaty, the Council acts on a proposal from the Commission, unanimity shall be required for an

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[1] Tsebelis and Kreppel (1995) trace the history of this article to the Treaty of Rome. In fact, it is repeated in all the treaties since then.

act constituting an amendment to that proposal." Moser recognizes this article in his presentation of the cooperation procedure but does not draw any strategic implications from it. Let us follow his Figure 1 (the case of similar preferences of the EP and the Commission) to see what he missed by ignoring this article.

The Council can always (both in the first and second reading) modify a Commission proposal by unanimity. Consequently, when the Commission introduces proposal v, while it is true that members 3–7 prefer v to the status quo, member 3 always (by construction) prefers point z in Moser's Figure 1 to both v and the status quo. Consequently, the coalition of members 3–7 is not a stable coalition. At least one of its members (and possibly more) prefer to enter in coalition with members 1 and 2 and try to have z voted instead.

Consequently, if the Commission proposes v, the members of the Council are faced with a coordination game. They prefer v over the status quo by qualified majority, but they also unanimously prefer z over the status quo; moreover, there is no clear ordering of the alternatives v and z. Both are equilibria in this game, which means that if all other necessary members have decided to vote for v, then the missing fifth member will vote for it, and if all other necessary members are determined to vote for z, then the last member will vote for it. Therefore, on the one hand, if member 3 makes a *credible* commitment that s/he will only vote for z, then z will be the outcome. If, on the other hand, member 7 makes a *credible* commitment that s/he will only support v, then v will be the outcome. Moser assumes (mistakenly) that if v is proposed, it will be the outcome. What happens, instead, is that *when the agenda setter makes a proposal of* v, *in fact s/he selects a lottery between* v *and* z *(and possibly, if there is no coordination, the status quo).*

Is there a way of guessing which one of these two or three outcomes is more likely to occur? A game-theoretic analysis would eliminate the status quo, since it is *not* an equilibrium, and would predict either z or v. Furthermore, such an analysis would introduce some additional criterion for equilibrium selection (refinement).[2] The standard refinements do not provide an answer in this case,[3] but an understanding of focal points and practices established in the Council is likely to be illuminating. [Readers familiar with game theory will notice that the argument that follows is equivalent with the claim that of the two sequential equilibria (that Moser and I propose), mine is supported by the off-equilibrium beliefs generated by the relevant literature (that the Council produces a unanimous decision whenever it can), while his collapses under the same set of beliefs.]

Of the three possible outcomes—the status quo (if there is no agreement), v (preferred to the status quo by a qualified majority), and z (preferred to the status quo by unanimity)—z has a focal point quality because it is always located in the middle between v and the status quo. In addition, there are frequent arguments in the literature about the preference for unanimous decision making in the Council (outcome z in our case). For example, it is well established that few formal votes are taken in the Council. Here is how a document by the European People's Party (EPP) Group (Christian Democrats) describes the situation:

> Informal votes are often held which reveal whether a qualified majority exists. If it does, the Council Presidency may simply say that a decision will be deemed to be taken unless anyone objects. Equally, *within the Council, significant efforts are often made to secure the widest measure of agreement*, to accommodate states which may not be able to stop a proposal being adopted but have strong concerns about particular points. *Everyone has a vested interest in a divisive vote being avoided.* As a result, few formal votes occur. However, without the certainty that a vote can be taken at the end of the day, there would be very little impulsion towards agreement (Infodoc, December 1995; emphasis added).[4]

What this passage and other similar accounts indicate is that if there is a point with unanimous support (such as z) it will be selected over nonunanimous points (like v). In its absence, an informal vote will be taken, and the prevailing option will be reported as the outcome unless some state wants to report its objection. So, if the agenda setter proposes v, the most likely outcome will be a unanimous decision by the Council to select z.

Here is the logic of my article in this simplified one-dimensional setting. If the agenda setter makes the proposal x, which the pivotal member 3 prefers over z (anything that the Council can decide by unanimity), then there is nothing that can lead 3 to defect from the coalition 3–7. Member 3 will not try to enter into coalition with 1 and 2 and upset the Commission's proposal. In other words, no alternative to x can emerge from *within* the Council. Consequently, *when the agenda setter makes a proposal of* x, *in fact s/he selects the outcome* x.

Now let us follow the agenda setter's decision making. S/he has to select between a lottery of v and z, where z (the outcome least preferred by the agenda setter) is more likely, or a sure event x. What is s/he going to do? The odds are overwhelming in terms of policy preferences that the agenda setter will go for x (the outcome predicted in Tsebelis 1994) instead of the lottery (the outcome predicted by Moser in his comment). If one adds to the equation institutional rivalries, according to which each institution prefers to see its own proposals accepted rather than disputed and amended, the odds become even more favorable for my analysis.

The reader has undoubtedly noticed that I am speaking in terms of probabilities and have established that Moser's argument is highly unlikely. Can I establish that it is impossible? The answer is no. If the agenda setter knows that unanimity is impossible (for example, if countries 1 and 2 have different ideal points and have made clear that they will not support anything other than their own ideal points), the agenda setter can propose v

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[2] For a word of caution against selection of equilibria on the basis of some formal criterion, see Kreps (1990).

[3] The refinements are perfect (Selten 1975), sequential (Kreps and Wilson 1982), or stationary (Baron and Ferejohn 1989).

[4] Similar arguments can be found in Peters (1992, 84–5), Dinan (1994, 254), and Miller (1995).

and have it accepted by the qualified majority of the Council.[5] But such cases will be rare, indeed. In conclusion, the agenda setter will make the proposal x in the second round, not because there was a unanimous vote by the Council in the first round (as Moser has me claiming), but for strategic reasons, in order to prevent a unanimous decision of z in the second round.

Since the logic of my argument prevails in the second round, let us see whether the Commission can do something about that in the first round, as Moser suggests. As I have said, there is no difference in the applicability of Article 189a(1) in the first or the second round, so in the first round the outcome would have been the same. But let as assume that the Council *cannot modify unanimously in the first round*, in order to follow Moser's logic closely. He claims my analysis "is based on an asymmetric assumption: While the EP acts strategically in the second reading, the Commission does not use its agenda setting rights effectively in the first stage." His argument, developed in the explanation of Result 1, is that even if the outcome would have been z in the second round, the Commission will prevent it by proposing v in the first round, with the result that then v cannot be upset in the second round (members 6 and 7 would vote against such a change).

At this point Moser has the strategic analysis backwards. Even under the (counterfactual) assumptions that we have made to favor his model, the outcome still would have been x not v. The reason is that v will be voted down by member 3 in the first round, so that x will be offered in the second round and accepted by a qualified majority. The reason for that outcome is the standard backwards induction reasoning of game theory: Strategic actors can look at the end of the game and will not accept anything less in order to terminate the game earlier.[6]

This point is important in order to examine Moser's claim that my analysis is either incorrect or incomplete. I have shown that Moser arrives at his results because he did not pay attention to Article 189a(1). The consequence is that his analysis is incorrect, not mine. The claims of incompleteness are also mistaken, because one has to analyze games backwards, and any (subgame perfect) equilibrium strategies in the game have to include equilibrium behavior in the last stage of the game. In other words, in order to draw conclusions about the cooperation procedure, one has to start from the second round, exactly as I did.

Moser's Result 1 is the most important theoretical disagreement between us. If correct, it would dispute not only the curvilinear property but also my whole line of argument and the concept of a *conditional* agenda setter. Indeed, the basic difference between Moser's unconditional agenda setting and my concept of conditional agenda setting is that in the first case the Council will revert to the status quo if it rejects the EP's proposal, whereas in the second case the Council has the option of reverting to a unanimously acceptable solution. This possibility significantly limits the powers of the agenda setter, whose proposal must make a qualified majority better off than any (unanimous) decision, otherwise s/he looses the agenda-setting power, which reverts to the unanimous Council. That is why I developed the concept of conditional agenda setting. Understanding the strategic implications of Article 189a(1) confirms my analysis of the second round of the cooperation procedure, and backwards induction confirms the same result for the first round.

Result 2, or, the Rule with Thousands of Exceptions

Moser predicts that in equilibrium the EP will have no influence on legislation. If the EP anticipates that some proposal will not pass, it will not make it; and if the EP's proposal would have passed, it will be offered by the Commission. So, in equilibrium there should be no amendments and certainly no EP influence. It is surprising that Moser brings this point up for debate, because the facts do not support his case. As he says, I have reported that for every four EP amendments, three are accepted by the Commission, and two of these are incorporated into law. What Moser does not present is the sheer numbers: Corbett et al. (1995, 199) report that from 1987 to 1993 the EP made 4,572 amendments, the Commission accepted 2,974, and the Council 2,219.[7] Depending on what one considers the major point of Moser's argument (no EP amendments, or amendments that will all be rejected by the Commission or will be rejected by the Council), there are 4,572, 2,974, or 2,219 exceptions to it. I do not understand why Moser insists on providing a model with so many exceptions as a benchmark.

Moser deserves credit, however, because he is the only person to confront the inadequacy of complete information models in providing an explanation for the facts. He at least tries to provide an ad hoc explanation,[8] specifically, that something changes between the first and the second round of the cooperation procedure. The change can occur in the position of the pivotal member of the Council, or in the position of the Commission, or in the position of the status quo. In any of these cases, the Commission can no longer amend the bill, so the only feasible amendments are introduced by the EP, and the Commission accepts them. Moser's ad hoc explanation reduces the number of exceptions between his theory and the facts, but not by very much. In the *first* round, the Commission accepted 2,499 of the EP's amendments and the Council 1,966.

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[5] I made a similar argument when I was trying to explain why European workplace health and safety regulation went outside the Pareto set of the Council (Tsebelis 1995). Under the prevailing rules, the Council would have been able to select unanimously an outcome located in its Pareto set. Yet, if one of the countries (say, the United Kingdom) refuses for its own domestic reasons to endorse any regulation on the matter, then the Council will adopt, by qualified majority, outcomes outside its Pareto set.

[6] I remind the readers with game-theoretic background that there are no time discount factors here.

[7] For the calculation of these numbers I assumed that there are no new EP amendments in the second round, which although mandated by the EP rules is not always followed in practice.

[8] Examples that do not discuss the empirical side are Steunenberg (1994) and Crombez (1996).

Of course, further analysis of these statistics is required.[9] In my own research, analyzing the legislative history of about 90 directives and roughly 1,400 EP amendments, I found that 12% were accepted by the Council in their entirety, 11% largely (more than half the text), 10% partially (less than half the text), and 20% with modifications. In total, 53% of EP amendments were accepted and the overwhelming majority in the first round. Although we lack systematic information concerning the *significance* of these amendments, we do know that some were important and that some were accepted in the first round of the cooperation procedure. For example, in another article (Tsebelis 1995, 73) I enumerate EP amendments that introduced significant changes in work place health and safety; I also show that some of these were accepted in the first round. Similarly, in examining environmental legislation, Kalandrakis (1996) divided EP amendments into significant and nonsignificant and found that of the 154 amendments examined, 82 were significant; that the acceptance rates were almost identical between amendments generally and those that were significant (0.54 in the first case, and 0.50 in the second); and that the major share of acceptance occurred in the first round (0.89 for all accepted amendments, 0.91 for significant accepted amendments).

What is interesting about these numbers is that, whether drawn from statistical studies or case analyses, they all tell the same story, one that is very different from Moser's model even *after* the addition of his verbal corrections. Moser's model expects no amendments to be accepted (indeed, he expects that amendments will not even be made), and his verbal modifications expect that no amendments will be accepted in the first round. In reality, *thousands of EP amendments are accepted, and the major part of them in the first round.*

Result 3, or, How to Force Open Doors

Moser's third result is constantly reported in my 1994 article, including the summary: "[The EP] can make a proposal that, if accepted by the Commission . . ." (p. 128). In fact, the three conditions for a successful EP proposal are (1) an absolute majority in the EP, by no means a trivial matter with existing attendance rates, (2) acceptance by the Commission, which happens three-quarters of the time, and (3) a status quo not far removed so that there is an area preferred by a qualified majority in the Council over any unanimous solution (Tsebelis 1994, 138). The reader can refer to my article to verify that acceptance by the Commission is always stated as a condition, so I will not pursue this point; instead, I will offer my explanation as to why Moser's results are so far off the mark.

MOSER'S ASSUMPTIONS

The two basic assumptions driving Moser's results are a single dimension of policy space and complete information. I will discuss the limitations imposed by each of these assumptions, but let me start with questioning their realism by discussing some little-known features of the famous catalytic converters case (SYN 115). I use it not because I believe it is typical but because it is instructive for our purposes.

In the directive concerning small cars, the difference between the EP, on the one hand, and the other two institutions (the Commission and the Council), on the other, was that the former wanted strict emission standards which could have been met only by the introduction of catalytic converters (20 grams/test for carbon monoxide and 5 grams/test for hydrocarbons and oxides of nitrogen), while the latter wanted more relaxed standards (30 grams/test and 8 grams/test, respectively).[10] In the first reading, the Commission and the Council rejected the EP's amendments in favor of their version, but in the second reading, the Commission and the Council decided to accept the amendments.

What I have not seen reported in the literature is that the Commission then proposed a compromise. It introduced its own (and the Council's) initial standards for a period of transition and accepted the EP's amendment thereafter (in fact, it even improved upon those standards by reducing the level of carbon monoxide emissions from 20 to 19 grams/test). The Council did not accept the two-period adjustment and voted to adopt the EP's standards in one step.

Some inferences to be drawn from this story are as follows. First, the story cannot be told in one dimension. Even if one wanted to do so,[11] the introduction of the two-step process by the Commission introduced a second dimension (time) into the debate. The Council was obviously closer to the EP in this second dimension (time) into the debate. The Council was obviously closer to the EP in this second dimension and did not see why a two-step process would be better than straightforward adoption. This story thus directly disputes Moser's single-dimension assumption. But there is more: Under complete information, the Commission never would have made an amendment which would not be adopted and which would lead the Council to line up with the EP.[12] Consequently, one single directive shows that both of Moser's fundamental assumptions are unrealistic (i.e., false). Now, I know that for some people having false assumptions is not considered a problem (Friedman 1953). I happen to believe that it is a major problem for a model, because when the fundamental assumptions are false, it does not matter what the conclusions are.[13] Indeed, once one ignores the facts and ventures to make Moser's counterfactual assumptions, there is no possible influence of the EP except in very limited ways, one of which Moser presents verbally, as well as some others I will present below. But there are many more reasons that such influence can occur; here are some examples.

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[9] I am currently undertaking such an investigation under the auspices of the NSF (grant #SBR 9511485).

[10] See EP amendment #3. For the original proposal, see OJC 262 10/10/88:89; for Commission revisions, see OJC 321 14/12/1988:7; for EP second reading, OJC 120 16/5/89; for Commission second revisions, OJC 134 31/5/89:8; for Council decision, OJL 226 3/8/89:1.

[11] Arguably, since it involves different technologies, it is already multidimensional.

[12] Cases in which the Council sides with the EP against the Commission are important for our discussion. In my study I found that it happens around 1% of the time.

[13] See Tsebelis 1990 (chapter 2) for extensive development of this point and the surrounding dispute over the question of "realism of assumptions."

In the first round of the cooperation procedure the EP makes amendments in *new dimensions* (different than the axis defined by the Commission and the Council). Some of these amendments may make the Commission better off, and it accepts them immediately (possibly with modifications), which may lead (because of conditional agenda setting) to the adoption by the Council. The Commission and/or the Council may reject EP amendments in the first round and wait until the second round to accept them. Why? Because the Commission and/or the Council may expect the EP to moderate its position in the second round and make a better (for them) offer. Since a simple majority is required for an amendment to pass on the floor in the first round but an absolute majority is required in the second round, the EP may, indeed, modify its position or fail to adopt the initial amendment in the second round; then again, it may simply reintroduce its first-round amendment. Again, this amendment may be accepted (conditional agenda setting) or not because there is no winning proposal (the Commission rejects it, or the Council modifies it unanimously). This story involves multiple dimensions and incomplete information, and it is (with all its variations) the modal history of the EP's amendments (at least in the roughly 90 SYNs I have seen). And this story with all its variations is consistent with my model (Tsebelis 1994) and inconsistent with Moser's.

But the EP also can make amendments along the dimension of disagreements of the Council and the Commission and have its proposals accepted for reasons different from the ones proposed by Moser. It is possible that the EP blackmails the Commission by threatening to delay or veto legislation (which it can do effectively with the support of a single government). In fact, "in 1981 the Parliament amended its procedures to enable its committees to postpone a vote on a Commission proposal until the Commission had taken a position on Parliament's amendments" (Judge 1992, 194). It is also possible that the Commission goes along with an EP amendment because it wants to maintain good relations with the EP, or because it wants to act as an honest broker (a point frequently raised in the literature) and is concerned about generating as little friction as possible within European institutions. Finally, it is possible for the Commission as a whole or for some actor within it to plant amendments through the EP. Here is how Judge (1992, 199) reports an interview with an official of DG XI (the Commission's directorate general in charge of the environment): "We will ask Parliament to do things, to look at things. If we need things to be changed in the Council then we might ask the EP to take an amendment on board. We then go back to Council and say 'Look this is what parliament is pressing for,' . . . This is a legitimate tactic on our part."

In short, EP amendments are not impossible, contrary to Moser's model. They do not occur only in the second round, as his verbal account expects. They happen frequently, and they are accepted by the thousands in both the first and the second round. They are the rule. Here is how the Commission characterizes the situation: "Since the Single European Act came into force on July 1, 1987, over 50 per cent of Parliament's amendments have been accepted by the Commission and carried by the Council. No national parliament has a comparable success rate in bending the executive to its will" [Commission Press release 15 December 1994; quoted in Earnshaw and Judge (1996, 96)]. The reason Moser arrives at mistaken conclusions is the two restrictive assumptions of his model, single dimension and complete information.

CONCLUSIONS

Moser's argument is inconsistent with both the Rome and Maastricht treaties and with the facts. Yet, his contribution deserves credit for two reasons. First, unlike other authors, Moser tries to bring his model to grips with the facts. To that effect, he identifies a mechanism that does occur in European legislation: the change of positions of some actors or the status quo. Change of preferences is a mechanism that is frequently neglected in the rational choice literature because the phenomena explained take place in a short period and because its intellectual quality is trivial (it is always possible to claim that actors changed behavior because they changed their mind). In the European context, however, when legislation may take years to be completed, and the composition of different deliberative bodies may alter from the first stage to the last, change of preferences or of the status quo is a plausible explanation. Moser brings this point out very forcefully.

Moser also deserves credit because he reinstates the fact that the Commission writes the first draft of legislation and, consequently, is also a conditional agenda setter in the cooperation procedure. While it does not have de facto exclusive jurisdiction as to the subject matters to be discussed,[14] it is the exclusive author of the initial proposal. In my original article I analyzed the second round and therefore did not underline this point. What I did underscore (and what I still consider the most important contribution of that article) are the powers of the *conditional* agenda setter. Unlike Moser's unconditional agenda setters, whose powers increase with the distance of the status quo from the positions of the Council, conditional agenda setters see their powers either change in a curvilinear fashion (in one or two dimensions) or decrease with the distance of the status quo from the Council. These properties have significant implications for the speed of European integration, implications described in the article and developed in more detail in subsequent publications (Garrett and Tsebelis 1996, Tsebelis 1995, Tsebelis and Kreppel 1995). I thank Moser for giving me the opportunity to revisit my article along with some of the literature written since.

Moser and I share the belief that the EU institutions are to a great extent responsible for the observed outcomes, such as the speed and directions of integration. Our institutionalist approach is based on deductive reasoning for the modeling part (as opposed to simple institution) and on systematic evaluation of the facts for the empirical part (as opposed to historical accounts of single events). Space constraints prevented me from pointing out these similarities and led me to focus on the differences.[15] Because our models come to sharper predictions (like points x and v in Figure 1) and are relevant to a set of facts (thousands of EP amendments), we are able to test our arguments better and present our evidence in more distinct contrast to each other than otherwise would have been the case. The extensive presentation of our disagreements, however, should not mislead the reader into the belief that they outweigh the points we have in common.

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[14] See Corbett, Jacobs, and Shackleton 1995; Judge 1992; and also Tsebelis 1994 for examples of the EP actually placing subjects on the agenda.

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[15] For a lengthier criticism of other approaches, see Garrett and Tsebelis (1996) and Tsebelis and Kreppel (1995) .

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The Claim of Issue Creation on the U.S. Supreme Court

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We argue that a variant of the sua sponte *doctrine, namely, the practice disfavoring the creation of issues not raised in the legal record, is a norm with substantial consequences for the U.S. Supreme Court. Without it, justices would act considerably more like legislators, who are free to engage in "issue creation," and less like jurists, who must wait for issues to come to them. Yet, McGuire and Palmer claim that justices engage in issue creation in a "significant minority" of their cases. We dispute this finding because we think it is an artifact of the way McGuire and Palmer collected their data. Indeed, for virtually every case in which they found evidence of issue creation, we show that the issue was actually present in at least one of the litigants' briefs. This suggests that justices may be policy seekers, but they are not policy entrepreneurs; and that briefs filed by third parties (such as amici curiae) are generally not a source of important issues considered by the Court.*

In 1963, the U.S. Supreme Court received seven cases for review which, in one way or another, touched on the subject of capital punishment. In none of these petitions did attorneys raise questions concerning the constitutionality of the death penalty; rather, all the claims hinged on procedural matters (e.g., challenges to the voluntariness of defendants' confessions, and not to their sentences per se).[1] Yet, prior to the conference at which the Court would decide whether to hear these cases, Justice Arthur Goldberg circulated a memo informing the justices that he would raise this question: "Whether and under what circumstances, the imposition of the death penalty is proscribed by the Eighth and Fourteenth Amendments to the U.S. Constitution." He recognized that none of the attorneys briefed this issue; nonetheless, he felt the Court should consider the question because he was convinced that "evolving standards of decency ... now condemn as barbaric and inhumane the deliberate institutionalized taking of human life by the state." Most of the Goldberg's colleagues were startled by his memo, complaining that it went well beyond their authority, that to implement his plan, the Court would have to proceed *sua sponte* (on its own, without prompting or suggestion). In the end, the justices not only rejected the memo's suggestion but also refused to hear the cases.

This story, and many others we could tell, suggests that a particular variant of the *sua sponte* doctrine, namely, the practice disfavoring the creation of issues not raised in the record before the Court,[2] is a norm: It establishes expectations, both in and outside the Court, about the way justices should behave; it tends to generate informal sanctions from other justices when it is not followed; and knowledge of it is widely held by members of the legal community (see, generally, Knight 1992). Thus, we can speculate on why the majority of the Court was so taken aback by Goldberg's memo and why it took the action it did:[3] Because the memo deviated from a norm the justices had come to accept, they "sanctioned" Goldberg by rejecting his invitation to reconsider the constitutionality of capital punishment.[4]

Framed in this way, the norm disfavoring the creation of issues is just as important as other Court norms scholars have uncovered, including the Rule of Four (see Perry 1991), the norm favoring respect for precedent (see Knight and Epstein 1996; cf. Segal and Spaeth forthcoming), and the norm of consensus (see Walker, Epstein, and Dixon 1988). In fact, it may be even more important. For if the norm of *sua sponte* did not exist, the justices would be free to raise any issue they so desired in any given case, even if attorneys had not briefed that issue. The implications of such behavior are of no small consequence. Most important, justices would act a good deal more like members of Congress, who are free to engage in "issue creation," and less like jurists, who must wait for issues to come to them. Thus, we could imagine rational, policy-seeking justices attempting, as a matter of course, to engage in strategic agenda setting—even after cases have been accepted, briefed, and argued—in order to manipulate case outcomes, just as do members of Congress over legislative proposals (see, e.g., Calvert and Fenno 1994).

If the Court were to operate free from a norm

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[1] We adopt this discussion from Epstein and Kobylka 1992, 42–3.

[2] Other variants of the *sua sponte* doctrine obligate a court to act, rather than prohibit a court from acting, "on its own." Most of these pertain to trial courts, such as the duty to conduct *sua sponte* inquiries into defendants' competence to stand trial (see Owel 1994), but a few implicate appellate courts (e.g., if such courts believe that they do not have jurisdiction to hear a particular dispute, they are obliged to so rule even if no party raised the issue). Here we focus exclusively on the variant preventing a court from acting without prompting, the norm disfavoring the creation of issues.

[3] Of course, there are other possible explanations, such as the Court's unwillingness to involve itself in a highly controversial issue area in the wake of *Brown* v. *Board of Education* (1954) (see Gray and Stanley 1989, 330).

[4] The Goldberg memo also shores up an important point about norms: Individual violations of them will occasionally occur. We take this up later in the article.

disfavoring issue creation, then other implications are easy enough to develop,[5] but the general point is simple: Without the norm the Court would no longer resemble a legal body in the way that scholars, attorneys, and jurists (not to mention Article III of the U.S. Constitution) contemplate such fora. One may even argue that a more-than-occasional deviation from this variant of *sua sponte* would undermine the Court's legitimacy. This follows from the fact that the legal community has come to believe that the legitimate judicial function involves review of the issues before it, not the creation of new issues (see, e.g., Llewellyn 1960). Thus, that community might reject as normatively illegitimate Court decisions that regularly and systematically create or "discover" issues (see, generally, Knight and Epstein forthcoming). As one scholar put it, "when the parties choose issues, there is little opportunity for judges to pursue their own agendas and, as a consequence, the proceedings are not only fairer, but are perceived to be fairer" (Krimbel 1989, 943). But, if the Court *departs* from this practice, it

> raises questions as to the impartiality of [its] actions, and such speculation tarnishes the Court's legitimacy. Litigant control of the issues is important to satisfy not only the parties, but society as well.... When the Court [discovers] issues that the litigants have not presented the Court erodes its credibility and trespasses on the soul of the adversarial system (p. 943).

Yet, in their 1995 *American Political Science Review* article, McGuire and Palmer—in a larger study on issue suppression and creation—claim to provide evidence of such a departure: In 18 of the 160 cases in their sample, the justices created issues that attorneys did not brief. As they put it (p. 699),

> in a significant minority of cases, the members of the Court provide authoritative answers to questions that have not been asked.... Such expansion ... of questions is not at all inconsequential because "via the manipulation of issues, the Supreme Court is exercising a role in the American political system for which it is generally not held accountable."

This claim not only suggests that the norm disfavoring issue creation fails to have the force that the legal community assumes but also that it is not a norm at all. Rather, in McGuire and Palmer's account, it is a "social practice" from which the justices feel free to deviate on a relatively regular basis.

In what follows, we take issue with McGuire and Palmer's finding, for we think it is an artifact of the way they collected their data. And, perhaps not so surprisingly, our results support this intuition: For virtually every case in which McGuire and Palmer found evidence of issue creation, we show that the issue was actually present in at least one of the attorneys' briefs. This leads us to conclude that the traditional view is correct: The practice disfavoring the creation of issues is, in fact, a norm; and it is one of several institutions that separates courts from legislators. That is, justices may be policy seekers, as so much of the literature characterizes them (see, e.g., Segal and Spaeth 1993), but they are not policy entrepreneurs, as are members of Congress or as McGuire and Palmer's account implies.

A REEVALUATION OF THE MCGUIRE AND PALMER DATA COLLECTION SCHEME

In reaching the conclusion that the Court occasionally creates or "discovers" issues, McGuire and Palmer compared the syllabi in the *U.S. Reports* (for orally argued cases decided during the 1988 term) with the "Questions Presented" in the briefs submitted on the merits by the parties. They claim that this approach enables scholars to assess the two forms of issue discovery: "(1) the justices may rule on an issue completely unique to a case ... or (2) the justices may specifically answer a question presented at a level of generalization greater than is necessary to cover that question" (p. 700).

Several flaws, in our view, exist in this scheme—flaws that may be sufficient to undermine the McGuire and Palmer claim that the Court creates issues in a "significant minority" of its cases. The first deficiency is straightforward enough: Comparing syllabi with questions is akin to mixing apples and oranges. Syllabi, while synopses, provide the highlights of the key points contained in the Court's opinion or judgment. In direct contrast, "Questions Presented" should, under the Court's own rules, "express concisely ... the circumstances of the case, *without unnecessary detail*. The questions should be *short* and should not be argumentative or repetitive" (emphasis added). Table 1, which displays the questions and the syllabus in one of McGuire and Palmer's issue-creation cases (*Barnard* v. *Thorstenn* [1989]), vividly illustrates this distinction. As we can see, attorneys in *Barnard* followed the Court's admonition that questions should be concise, while the Court reporter took the opposite tack in the syllabus, summarizing virtually all the key points. More generally, for those cases in which McGuire and Palmer found issue creation, the ratio of points raised in the syllabi to "Questions Presented" is more than 2:1.[6] Surely, these data, not to

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[5] For example, we know from the congressional literature that legislators seek to make good policy and to gain reelection but, in so doing, they face considerable uncertainty about the substantive and political ramifications of alternative courses of action. Interest groups, by lobbying and mustering grassroots pressure, provide valuable information on the views of organized and intense constituents (see, e.g., Hansen 1991) and about the policy consequences of their actions, thereby reducing risk for legislators (Krehbiel 1991).

In a legal world unconstrained by a norm disfavoring the creation of issues, justices would face similar uncertainty—not about their constituents but about the actions their colleagues might take and about the political, economic, and social ramifications of their decisions. Yet, the results might be different from what occurs in Congress; that is, we might predict a decline in the importance of attorneys and interest groups, rather than an increase in their value to the justices. To see why, consider a world in which justices could transform a case in which attorneys raised First Amendment claims into one involving search and seizure or privacy or capital punishment, and so on. In such a world, attorneys, interest groups, and other "lobbyists" would have difficulty identifying even the proximate grounds on which the Court would decide the case. And, even if they could, they would be unable to load up their submissions with the range of possible issues in light of Court rules limiting the length of briefs. Of course, justices—realizing that attorneys were not in a position to provide them with useful information—might simply disregard written submissions.

[6] The total number of "Questions Presented" in the 18 cases was 40 (mean = 2.2 questions per case); the total number of points (issues)

TABLE 1. A Comparison of the Syllabus (Excerpted Version) and Questions Presented in *Barnard* v. *Thorstenn* (1989)

Syllabus in *U.S. Reports* (excerpted)	Question in Petitioners' Brief	Question in Respondents' Brief
1. The Court will not exercise its supervisory power in this case, since both the nature of the District Court and the reach of its residency requirements implicate interests beyond the federal system. 2. Rule 56(b)'s residency requirements violate the Privileges and Immunities Clause, since none of the justifications offered in support of the requirements are sufficient to meet petitioners' burden of demonstrating that the discrimination against nonresidents is warranted by a substantial objective and bears a close or substantial relation to such an objective. a. Petitioners' contention that the geographical isolation of the Virgin Islands, together with irregular airline and telephone service with the mainland, make it difficult for nonresidents to attend court proceedings held with little advance notice, is an insufficient justification. b. The District Court's finding that the delay caused by trying to accommodate the schedules of nonresident attorneys would increase the massive caseload under which that court suffers is an insufficient justification. c. Petitioners' claim that delays in the publication of local law require exclusion of nonresidents because they will be unable to maintain an adequate level of professional competence is unpersuasive. d. The contention that the Virgin Islands Bar Association does not have the resources and personnel for adequate supervision of the ethics of a nationwide bar membership is not a sufficient justification, since increased membership brings increased dues revenue, which presumably will be adequate to pay for any additional administrative burdens. e. Also unavailing is petitioners' argument that the residency requirements are necessary to a strict and fair application of Local Rule 16, which requires each active bar member to be available to accept appointments to appear on behalf of indigent criminal defendants, and which is interpreted by the District Court to require that only the appointed attorney may appear on behalf of the defendant.	Whether the decision in *Frazier* v. *Heebe*, 107 S.Ct. 2607 (1987), prohibits a federal court in the unincorporated territory of the U.S. Virgin Islands from requiring residency as a requisite for the practice of law?	Was the court of appeals correct in invalidating a district court rule requiring that lawyers reside in the Virgin Islands in order to be licensed to practice law here?

mention the Court's own rules, suggest that we ought to consider issues raised in the balance of briefs before reaching any strong conclusions about the existence (or lack) of the norm disfavoring issue creation. In other words, one could hardly claim that the Court was discovering issues if those very issues were located in other sections of the brief.

The second flaw, or at least a potential flaw, concerns the scope of McGuire and Palmer's search for issues: They limited their data collection to petitioners' and respondents' briefs on the merits, whereas the Court receives many other submissions, including reply briefs and briefs amicus curiae. Recent research (Spriggs and Wahlbeck 1995) highlights the problem with the McGuire and Palmer strategy as it pertains to amici curiae: In nearly 16% of all cases, do "friends of the Court" present arguments not found in any other brief.[7] Surely, if some party—third or otherwise—raises an issue, then that issue is part of the legal record before the Court. Accordingly, it would not violate the norm of *sua sponte* for the justices to consider it; after all, the norm only constrains the Court from reaching issues "on its own, without prompting or suggestion." The same logic would hold with even greater force for reply briefs.

ANOTHER LOOK AT ISSUE CREATION

These concerns about McGuire and Palmer's data collection strategy guided our research. We began by obtaining the syllabi in the 18 cases in which McGuire and Palmer claimed that the Court "discovered" issues, that is, violated the norm disfavoring issue creation.[8] As the Appendix shows, the syllabi contained 91 points (including subpoints) or "issues" collectively addressed by the opinions in the 18 cases. Next, we invoked the

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raised in the syllabi was 91 (mean = 5.1 syllabi points per case). To compute the figures for the briefs, we took the average number of questions raised by the parties to the case.

[7] McGuire and Palmer recognize that amici curiae do, in fact, raise arguments not contained in the parties' briefs; indeed, they recount the classic story of the ACLU's role in *Mapp* v. *Ohio* (1961). Yet, for reasons that are unclear, they chose to ignore amici.

[8] We thank Kevin McGuire for providing us with a list of these cases.

McGuire and Palmer procedure by comparing the points listed in the syllabi with the "Questions Presented" in the briefs of the parties.

For those points not found in the "Questions Presented," we departed from the McGuire and Palmer strategy and examined the body of the briefs. From there, we turned to amicus curiae submissions for any remaining points. As our discussion above suggests, this revised procedure allows for a more accurate account of issue creation than does the McGuire and Palmer approach. For, if an issue is raised in the body of a brief (even if it is not a "Question Presented"), we can hardly say that the Court is engaging in issue creation. Less clear, perhaps, are cases in which the issues arise only in amicus briefs, but this distinction does not affect our results, as we shall see.

Before turning to our findings, however, three caveats about our research procedure are in order. First, as noted earlier, we (as did McGuire and Palmer 1995, 700) considered only those issues presented in the case syllabi. Needless to say, Supreme Court opinions, which can be more than 100 pages in length, might well contain issues that are not summarized in the syllabi. Thus, neither our design nor McGuire and Palmer's can tell us whether the Court brings up minor or secondary issues on its own; rather, we can only determine whether the *major* issues decided by the Court derive from the parties' briefs. Even so, there are clear advantages to relying on the syllabi, not the least of which is that they provide reliable roadmaps to Court decisions. In other words, while two independent scholars examining the contents of a decision might disagree over whether the opinion writer raised a particular issue, no such dissentience can occur over the points listed in the syllabus.

Second, we repeat the warning from footnote 2 that the doctrine of *sua sponte* applies in reverse when it comes to questions of jurisdiction and standing. The Court has an affirmative obligation to resolve such questions even if they are not raised by litigants. This poses few problems with regard to jurisdiction, for the parties' briefs—as a matter of course—address the Court's ability to hear cases.

Finally, although coding the data (that is, comparing syllabi points with the contents of written briefs) requires some judgment, most cases are clear enough to be beyond any controversy. Where our results might engender debate, we justify our conclusions in the Appendix. This was an especially necessary step for those cases in which dissenting or concurring justices claimed that a particular issue was not properly before the Court. As our notes to the Appendix suggest, we found ample evidence in the briefs themselves to refute such claims, suggesting that the dissenting or concurring justice may have had substantive reasons for making them.

RESULTS

Following our research strategy, we begin by comparing points raised in the syllabi with the questions listed in the parties' briefs. Our findings, displayed in Table 2, indicate that 44% of the 91 issues raised in the syllabi were present in the "Questions Presented." Also note that for three of the cases in which McGuire and Palmer alleged issue creation, a replication of their approach indicates that this was not the case. In other words, for nearly 17% of the cases all the points in the syllabi appear in the "Questions Presented."

TABLE 2. Location of Issues in Briefs

Location of Issue Raised in the Syllabus	Cumulation of Issues %	Issues N	Cases %	Cases N
Question sections of merits briefs filed by the parties	44.0	40	16.7	3
Other sections of merits briefs filed by the parties	97.8	89	88.9	16
Reply briefs filed by the parties	98.9	90	94.4	17
Amicus Curiae briefs	98.9	90	94.4	17

Note: The total number of issues (points in the syllabi) is 91; the total number of cases is 18. So, of the 91 total issues raised in the syllabi, 44% (40 of the 91) showed up in the question section of the briefs; for 3 of the 18 cases (16.7%) all the issues in the syllabi appeared in the question section.

For those syllabi points that we could not locate in the questions, we turned to the balance of the parties' main briefs. As Table 2 shows, this procedure unearthed the vast majority of issues raised in the syllabi (97.8%), and it covered 16 of the 18 cases. For the remaining two points and two cases we examined the reply briefs filed by the parties. From these, we were able to cover one of the two syllabi points and one of the two cases (see Table 2).

That left us with the following point, listed in the syllabus to *City of Richmond* v. *J. A. Croson Co.* (1989):

> The "evidence" relied upon by JUSTICE MARSHALL's dissent, the city's history of school desegregation and numerous congressional reports, does little to define the scope of any injury to minority contractors in the city or the necessary remedy, and could justify a preference of any size or duration.

Since this issue implicates a dissenting opinion filed in the case, it is understandable why we were unable to locate it in any of the submissions, including our last source—briefs amicus curiae.

DISCUSSION

The results of our reanalysis of the McGuire and Palmer study are easy enough to summarize: With one highly explicable exception, the parties raised all the points covered in the syllabi. Thus, at least for the cases included in McGuire and Palmer's sample, the Court virtually never created a major issue that was not part of the existing record.[9]

On that note, we could end our analysis of the claim of issue creation on the U.S. Supreme Court. But our investigation, we believe, tells us much more about the formulation of Court opinions. First, our study provides evidence, albeit of a limited nature, to refute the proposition that amicus curiae briefs contribute to the development of the law by raising key issues unaddressed by the parties (see, e.g., Cortner 1975; Epstein and Kobylka 1992). To be sure, we do not, and cannot, take issue with Spriggs and Wahlbeck's (1995) study showing that amici raise *unique* arguments. But, based on our results, we can say that it is the rare case in which amici raise *important* arguments (i.e., arguments highlighted in the syllabi) that the parties themselves fail to raise. Indeed, in our study, this never occurred.

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[9] Still, it is worth noting, petitioners and respondents do not always *fully* brief all the issues decided by the Court; and, occasionally, even the justices disagree over whether an issue was fully briefed. For more on these points, see the Appendix.

Taken on its face, this result leaves us with many questions about amici participation on the merits of cases. Consider but one, emanating from Caldeira and Wright's (1988) research on amicus briefs filed on certiorari. Based on predictions congressional scholars developed from signaling games and the informational role of interest groups (e.g., Krehbiel 1991, Wright 1990, 1995; see also our footnote 5), Caldeira and Wright posit that amicus briefs filed on certiorari reduce the Court's uncertainty about the importance of cases—a prediction that their data support. In the aftermath of their study, some have argued that scholars ought to apply a similar logic to amicus briefs filed on the merits: They may lessen uncertainty over the importance of decisions for nonlitigants, the consequences of holdings for various constituencies, the feasibility of implementation, and so forth (see, e.g., Caldeira and Epstein 1994). But since our study, as limited as it is, finds that amici arguments largely replicate those of the parties on the *key* points, we wonder whether this is as fruitful an avenue of research as it was for certiorari decisions. At the very least, scholars might ask whether our results hold up against larger samples and, if so, why such an apparent difference exists between the effect of briefs filed on certiorari and those filed at the plenary review stage.[10]

A second set of implications of our study, of course, centers on the norm disfavoring the creation of issues. Most obviously, we question the claim implicit in McGuire and Palmer's study, namely, violation of the norm occurs in a nontrivial number of cases. At least among important issues (again, those summarized in the syllabi), our findings lead to quite a different conclusion: Justices evince behavior that is consistent with the existence of this variant of the *sua sponte* doctrine. At the same time, however, we would be wrong to suggest that violations of the norm never occur. We have already recounted the story of Goldberg and his death penalty memo, and in conducting this research we came upon yet another—the McGuire and Palmer "issue creation" case of *Patterson* v. *McDean Credit Union* (1988). The Court granted certiorari in *Patterson* to consider whether 42 U.S.C. §1981 provides a remedy for racial harassment (*Patterson* 1987, 814). Yet, after oral arguments, the justices requested attorneys to brief a question which no party or amicus curiae had raised: "Whether or not the interpretation of 42 U.S.C. §1981 adopted by this Court in *Runyon* v. *McCrary* should be reconsidered" (*Patterson* 1988, 617). To some legal scholars (e.g., Cook 1928; Degnan and Louisell 1956; Krimbel 1989), this kind of request is a clear violation of the *sua sponte* norm: When courts ask for rearguments on matters the parties did not raise, they engage in issue creation.

While we are sympathetic to this claim, two factors dampen our enthusiasm. For one thing, if the Court did not respect the norm disfavoring the creation of issues, then it merely would have reconsidered *Runyon* without asking for rearguments; in other words, if the Court could discover issues, it could surely reexamine past cases *sua sponte*. Seen in this way, the *Patterson* order may lend further support for the existence of the norm of *sua sponte*, rather than ammunition to refute it. Second, the request for reargument in *Patterson* elicited, not unlike the Goldberg memo, a highly negative response: Four justices dissented, asserting that "neither the parties nor the Solicitor General [as an amicus curiae] have argued that *Runyon* should be reconsidered" (*Patterson* 1988, 617); newspapers and magazines took aim at the Court's majority (see, e.g., Jacoby and McDaniel 1988); legal scholars deemed the order an example of brute activism (see, e.g., Krimbel 1989); and, at the end of the day, the Court did not overrule *Runyon*. Of course, we do not claim that the decision to retain *Runyon* was causally connected to the overwhelmingly negative reaction to the reargument order; yet, because of the "fuss" following the request in *Patterson*, legal scholars have speculated that "it may be a long time before the Court requests rehearing *sua sponte*" (Krimbel 1989, 933).

Thus, we end where we began: It is no great mystery why Goldberg's memo so disturbed his colleagues or why the *Patterson* order so troubled the legal community. In both instances, justices were perceived as attempting to violate the norm disfavoring the creation of issues. While occasional deviations from the norm are not unexpected, regular and systematic departures should be rare. Since this prediction holds, as our study demonstrates, at least one important distinction between legislators and justices remains firmly intact.

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[10] Certainly, it is possible that the Court might be influenced by evidence, presented in amicus curiae briefs, of the political, economic, or social consequences for a particular "constituency" but would think it inappropriate to issue legal decisions explicitly based on those consequences. The general point, though, is the same one we raised in the text: Our research strongly suggests the need for more systematic study of amicus curiae briefs filed on the merits of cases.

APPENDIX

TABLE A-1. Coding of McGuire and Palmer's 18 "Issue-Creation" Cases

Case/ Syllabus Issue	Where Can Syllabus Issue Be Found? Appellant/Petitioner Brief: Question Section	Appellant/Petitioner Brief: Elsewhere	Appellee/Respondent Brief: Question Section	Appellee/Respondent Brief: Elsewhere
87-107 *Patterson*				
1.[a]	Question 1 (reargument)			
2.	Question 1			
2a.		pp. 20–1		
2b.				pp. 8–9
2c.				p. 22
3.	Question 2			
3a.		p. 39		
3b.		p. 93		
87-154 *DeShaney*				
1.	Question 2			
1a.[b]	Question 2			
1b.		p. 11		
1c.		p. 9		
87-470 *Fort Wayne*				
2.[c]			Questions 2 and 3	
2a.			Question 1	
2b.	Question 4			
3.	Question 1			
87-614 *Sappenfield*				
1.[d]		p. 2		
2.	Question 1B			
2a.	Question 1			
2b.			Question 8	
2c.	Question 2			
2d.[e]	Question 3			
87-996 *Coit*				
1.	Question 1			
1a.		p. 7		
1b.		p. 7		
1c.		pp. 23–4		
2.				p. 17
2a.			Question 2	
2b.				p. 15
2c.				p. 14
87-998 *Richmond*				
1.			Question 1D	
1a.			Question 1C	
1b.			Question 1B	
1c.				
1d.				p. 29
2.			Question 2	
87-1022 *NYC Board*				
1.	Question 1			
1a.	Question 1			
1b.	Question 2			
1c.		p. 23		
87-1160 *Duquesne*				
1.[f]		p. 2		
2.	Question 1			
2a.	Question 2			
2b.			Question 2	
2c.			Questions 4 and 5	
2d.			Question 7	

continued

TABLE A-1. Continued

Case/ Syllabus Issue	Where Can Syllabus Issue Be Found? Appellant/Petitioner Brief: Question Section	Appellant/Petitioner Brief: Elsewhere	Appellee/Respondent Brief: Question Section	Appellee/Respondent Brief: Elsewhere
87-1346 *Bonito Boats*				
1.		p. 16		
1a.		p. 16		
1b.		p. 12		
1c.	Question 1			
1d.		p. 3 (reply brief)		
87-1372 *Argentine*				
1.	Question 2			
1a.		p. 12		
1b.		p. 22		
1c.	Question 2			
1d.				p. 16
1e.	Question 3			
1f.[g]			Question 3	
1g.				p. 16
87-1428 *Lorance*				
1.			Question 1	
87-1848 *Dallas*				
1.	Question 1			
2.[h]		p. 5		
87-1939 *Barnard*				
1.		p. 8		
2.		p. 31		
2a.		pp. 10–1		
2b.		p. 12		
2c.		p. 9		
2d.		p. 14		
2e.		p. 14		
87-2008 *Virgin Islands*				
1.		p. 8		
2.		p. 31		
2a.		pp. 10–1		
2b.		p. 12		
2c.		p. 9		
2d.		p. 14		
2e.		p. 14		
88-317 *Duckworth*				
1.			Question 1	
1a.	Question 1			
1b.				p. 6
1c.				p. 7
1d.				p. 6
88-357 *Maleng*				
1.	Question 1			
88-556 *Browning*				
1.	Question 1			
1a.		p. 17		
1b.		p. 7		
2.		p. 27		
3.		p. 48		
88-782 *Justice*				
1.		p. 10		
1a.	Question 1			
1b.		p. 9		
1c.				pp. 9–10

Note: The coding procedure was to attempt to locate the syllabus point in the question section of the appellant/petitioner brief. If located, coding ends; if not, attempt to locate the syllabus point in the question section of the appellee/respondent brief. If located, coding ends; if not, attempt to locate the syllabus point in the balance of the appellant/petitioner brief. If located, coding ends; if not, attempt to locate the syllabus point in the balance of the appellee/respondent brief. If located, coding ends; if not, attempt to locate the syllabus point in appellant/petitioner reply brief. If located, coding ends; if not, attempt to locate the syllabus point in appellee/respondent reply brief. If located, coding ends; if not, attempt to locate the syllabus point in a brief *amicus curiae*.

continued

TABLE A-1. Continued

[a] See p. 849 of this article.

[b] In his dissent, Justice Brennan claimed that the majority decided the Due Process Clause "creates no general right to basic governmental services" (1989, 203). He then correctly noted that this question was not briefed on the merits. To Brennan's charge of issue creation, we offer three responses. First, we note that the syllabus did not raise this point, dealing instead with the more specific question of protective services, which the litigants clearly raised in their briefs Second, Brennan's statement does not appear to characterize precisely what the majority decided. The majority wrote that "the Due Process Clause" generally confers no affirmative right to governmental aid" (1989, 196). This, we believe, is less sweeping than Brennan's pronouncement. Finally, the majority's claim (whether raising a major issue or not) appears almost verbatim on page 12 of the respondent's brief ("The Court has . . . repeatedly held that the Constitution does not provide an affirmative right to governmental aid.").

[c] Point 1 pertained only to the companion case 87–614, as did points 2c and 2d.

[d] The syllabus discussed jurisdictional problems under 28 U.S.C. 1257. The petitioner explicitly claimed jurisdiction under that section (and, thus, meets our requirements) but did not elaborate. Elaboration aside, the Court did not create the issue.

[e] Point 3 pertained only to the companion case, 87–470.

[f] The syllabus discussed a jurisdictional question raised, but not elaborated upon, in the petitioner's brief. Adding to the controversy, the majority opinion noted it was responding affirmatively to the jurisdictional question even though that question was not "discussed" in the briefs. Again, though, the Court did not create the issue; the petitioner clearly raised it first.

[g] Justice Blackmun's concurring opinion did not reach the question of whether any of the FSIA's exceptions to foreign sovereign immunity apply to this case because the court of appeals did not decide it—a point the majority disputed (1989, 439, fn. 6). Blackmun further claimed that the issue "did not receive full briefing" (1989, 443). The comment, however, suggests that the issue was in fact briefed, and that is the case (e.g., Petitioner's Brief, pp. 5, 9). Moreover, as the syllabus noted, it was the district court which initially determined that "none of the exceptions enumerated in the FSIA applies to the facts of this case." Thus, under the circumstances, it would be rather difficult to claim that issue was not part of the legal record.

[h] This case involved a municipal statute that restricted admittance to certain dance halls to persons aged 14 through 18. In a footnote to his concurring opinion, Justice Stevens claimed that the equal protection argument raised by the majority was not briefed before the Court. This is not quite correct. The petitioner, after arguing that there was no First Amendment right of association involved, claimed that the age limit met the appropriate standard of review: rational basis. The brief further argued that even under a strict scrutiny standard a compelling state interest was served. This, of course, is the classic language of an equal protection argument. The respondent replied that the petitioner made "no serious equal protection argument" (Petitioner Reply Brief, p. 3). Whether the argument was serious or not, the Court did not create the issue.

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Issues, Agendas, and Decision Making on the Supreme Court

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In the process of agenda setting, the U.S. Supreme Court is limited to selecting from among only those cases brought before it. Despite this limitation, the justices possess considerable discretion and can reshape the issues in a case as a means of advancing their policy preferences. With data drawn from the Court's opinions, we find that, over the past twenty-five years, the justices have evinced a frequent willingness to expand the issues on their plenary docket and resolve questions not formally presented by the parties. We conclude that, notwithstanding informal norms that disapprove of this practice, issue fluidity is an important component in a continuous program of agenda building.

In 1972 the U.S. Supreme Court heard oral arguments in two cases that, in one way or another, touched on the subject of abortion. In neither of these cases did attorneys raise questions concerning the point at which, during a pregnancy, the state had a legitimate interest in regulating or proscribing abortions; rather, all the claims hinged on other matters (e.g., whether the appellant presented a justiciable controversy, and whether the Constitution guarantees the right to abortion).[1] Yet, during the argument, several of the justices made repeated inquiries regarding the application of abortion laws at different stages of fetal development. Sarah R. Weddington, the lawyer appearing on behalf of Jane Roe, recognized that neither party had briefed this issue: "There are some states that now have adopted time limits. Those have not yet been challenged. And, perhaps that question will be before this Court.... But that's really not before the Court in this particular case." Ms. Weddington's response to these questions, however, "underscored her strategic naiveté. Whether the issue was formally 'before the Court' was far less significant than the fact that the justices were obviously concerned about it" (Epstein and Kobylka 1992, 191–2).[2] In the end, as is well known, the Court's opinion in *Roe* v. *Wade* (1973) divided pregnancy into trimesters, delineating the state's increasing regulatory interest in each successive period. Arguably the case's "most significant [question] was at what point states can become involved in the abortion decision" (p. 197); interestingly enough, it was never posed by the parties.

This story, and many others we could tell, serves to illustrate the practice of issue fluidity on the Supreme Court, that is, the ability of the justices to expand or contract the range of issues presented by the parties in a case (Ulmer 1982). In particular, the *Roe* decision highlights the Court's substantial capacity to engage in issue expansion, to resolve authoritatively legal questions not raised by the parties to litigation.[3] Thus, as the justices set about making decisions on the merits, it is quite possible that, in the end, their written opinions will focus on concerns that go beyond the boundaries of the written briefs. The reason the Court expanded the range of issues in *Roe* is not difficult to discern; as Epstein and Kobylka (1992) correctly note, it was merely what a majority of its members were disposed to do. What enabled them to engage in this behavior is not particularly baffling either; the justices possess an enormous degree of discretion in the disposition of their plenary docket (Segal and Spaeth 1993). Is *Roe* representative of a more general pattern of decision making on the Supreme Court?

With our interest kindled by illustrative cases such as this one, we attempted to answer this question (McGuire and Palmer 1995). Briefly, our analysis of the 1988 term of the Court found evidence of issue expansion in roughly 10% of the cases, and, consistent with our expectations, the practice appeared to be a proactive decision driven largely by discretionary factors. Ironically, in the critique of our analysis by Epstein, Segal, and Johnson (1996, 848), one of the scholars who drew our attention to the potential importance of this behavior now seemingly disavows the prior compelling research in Epstein and Kobylka (1992). The authors of the critique argue that the justices "virtually never" decide an issue not presented by the parties; in other words, in the process of decision making in the Court, cases such as *Roe* v. *Wade* are wholly uncharacteristic events. Notwithstanding the extraordinary stochastic case in which the Court resolves issues *sua sponte*, the justices are instead bound by a legal norm that limits the Court to deciding only the questions presented by the parties.[4] In short, according to our critics, issues simply are not fluid in the U.S. Supreme Court.

To the contrary, we are inclined to view the expansion of issues at the merits as an inevitable (and perhaps even necessary) part of formulating an agenda in the high

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[1] See *Roe* v. *Wade*, Brief for Appellee 1973, 6–7.

[2] Although this exchange took place during the reargument of the case in fall 1972, it was a matter of similar concern during both the original argument and its subsequent conference discussion (see Epstein and Kobylka 1992, 178–86; Schwartz 1988, 83–92).

[3] Given that our work was derived directly from Ulmer (1982), we initially adopted his specific terminology for this behavior, referring to it as "issue discovery." In light of the criticism it here receives, we are convinced that this term is a bit too value-laden, connoting—however indirectly—an inappropriate behavior. Ulmer also employed other descriptors, including "issue expansion," and we now propose to substitute it as a formal alternative (see also Pacelle 1991). The reason is that, at least to us, "issue expansion" seems to imply greater neutrality regarding the advisability of its practice.

[4] When the Supreme Court decides an issue *sua sponte*, it does so "of ... its own will or motion; voluntarily; without prompting or suggestion" (Black 1979, 1277).

court, a position we develop here more fully. To that end, we argue that the manipulation of issues at the merits stage is but one part of what is principally an ongoing process of agenda setting. In addition, we marshal evidence suggesting that, at least for the past two decades, it has occurred persistently. More important, we find that, far from validating the doctrine of *sua sponte*, ours is an interpretation that is largely shared by the justices themselves.

EXPANDING ISSUES ON THE JUDICIAL AGENDA

Broadening the range of issues in a case has a long history in the Supreme Court. At least as far back as *Marbury* v. *Madison* (1803), the justices have been receptive to the idea of injecting new issues into the cases brought before them. In that instance, the only question before the Court was whether the justices had the authority to issue the writ of mandamus against Secretary of State James Madison; the Court's right to exercise judicial review was not raised by either party (Schwartz 1993, 40). "Indeed, in *Marbury* . . . the argument of Charles Lee on behalf of the applicants . . . reproduced in the Reports of [the] Court where anyone can see it . . . devotes not a word to the question of whether [the] Court has the power to invalidate a statute duly enacted by the Congress. Neither this ground of decision nor any other was advanced by Secretary of State Madison, who evidently made no appearance" (*Monell* v. *New York City Department of Social Services* 1978, 717). Since then, the justices have frequently transcended the formal issues presented by litigants, using a variety of cases as vehicles to amplify their policy ambitions. Drawing from such diverse issues areas as antitrust, criminal law, libel, and school desegregation, the Court has, in case after case, been willing to speak to substantive legal issues that were not contemplated by their participants (Ulmer 1979, 1982). Why would such transformations of issues take place? Why would the justices be willing to decide important questions not formally presented? To answer these questions, it is profitable to see the Court's potential to remold the questions brought before it as a part of a larger process of establishing an agenda.

One of the most important lessons of recent years that has emerged from the literature on the Supreme Court's agenda setting is that its mechanics are highly continuous. Far from just selecting some cases and rejecting others, the Court follows a more sequential mode of operation in which cases are gradually siphoned off, as the justices make increasingly more exact judgments at each successive phase. Thus, for example, based on such general indicators as dissent in a lower court or the presence of a civil liberties issue, some of the myriad filings are placed on the Court's list of candidates worthy of formal discussion; responding to more distinct indices, such as actual conflict or the United States as a petitioning party, some petitions on the discuss list are, in turn, ultimately granted formal consideration (Caldeira and Wright 1990, Perry 1991). Since these cases, however, are only a convenient legal architecture within which to resolve the more specific issues they contain, it seems highly plausible that, as they are being resolved, those issues may be subject to some amount of reshuffling—some further degree of refinement—at the hands of the justices. In this light, then, the expansion of issues at the merits stage can be seen as merely one in a series of steps that constitutes the Supreme Court's agenda setting.

However constituted, there is no doubt that the Court's agenda is driven in no small degree by the policy preferences of the members. In fact, scholars have long recognized that the justices' attitudes are manifested in the selection of cases (Armstrong and Johnson 1982, Baum 1977, Songer 1979, Ulmer 1978). Accordingly, one might suspect that, if issue transformation were a natural consequence of establishing agenda priorities, those attitudes would be in operation in the treatment of specific issues as well. Yet, because Epstein, Segal, and Johnson (1996) are unwilling to consider the transformation of issues on the merits as a possible component of agenda building, they conclude that any such modification violates a sacrosanct norm of judicial restraint, casting doubt upon the Court's institutional legitimacy. This is a rather puzzling perspective, since the literature's portrayal of issue fluidity has precisely the opposite orientation; issue expansion is generally conceded as an illustration of how policy-minded Court members may realize their goals within the limits unique to the enterprise of judging:

> In an important sense, the justices of the Supreme Court are policy entrepreneurs, who seek to fulfill their policy goals through their case selection policies and their decisions on the merits of the issues. . . . Members of the Court who desire to make public policy may not have the proper cases necessary to make policy at the appropriate time. In such instances, purposive justices can manipulate cases before them to pursue their designs. This process [i.e., issue expansion] mitigates the problems created by the fact that the Court is not a proactive institution (Pacelle 1991, 31–2).

In other words, because the justices are unable to create cases and are bound to resolve only those conflicts brought before them, broadening the range of issues within an existing case sometimes becomes the only legitimate route by which they can realize their policy preferences. It is precisely *because* the Court is a reactive institution that issue expansion becomes a requisite part of agenda formulation in the judicial arena. In deciding questions of law, the justices can surely paint with a fairly broad brush on the canvas supplied by a case, but they must still respect the frame that circumscribes it.

Clearly, then, there are contextual limits posed by individual cases; indeed, the Court's rules codify those lines of demarcation. Still, there can be no question but that the justices have reserved—again, quite explicitly within their rules—the right to remold a case to suit their purposes, a provision that "has been invoked with some frequency to dispose of cases on grounds not raised or even noticed by the parties, let alone the courts below" (Stern et al. 1993, 346). In this respect, the general guideline that new questions ought not be considered is by no means unusual; most of the limits that rein the Court's policymaking are not exempt from breach at the whim of its membership. Jurisdiction and standing, for example, are obvious threshold requirements that must be overcome in order for the justices to review a case, but these criteria "are no less subject to judicial manipulation than are those governing other areas of the law" (Segal and Spaeth 1993, 206). Similarly, the norm against deciding issues not presented is entirely of the Supreme Court's own creation. Consequently, "in determining whether to consider issues not raised in a petition, the Court not only has broad discretionary power, but . . . it can exercise that discretion so as to permit it to decide the issue it wants to decide. . . . [I]t reflects the Court's discretionary authority to dispose of cases in what it determines to be the most sensible and reasonable way" (Stern et al. 1993, 345–6).

Quite apart from the Court's leading practitioners, to say nothing of the justices themselves, political scientists for some time now have recognized the incidence of issue fluidity. In *Elements of Judicial Strategy*, Murphy (1964, 29–30) observed that the justices "have found issues in the case which opposing counsel did not see and have decided those issues without allowing argument." Leading examples of issue expansion have since figured prominently in individual cases studies (see, e.g., Cortner 1981, Epstein and Kobylka 1992) as well as textbooks on both the Court's processes and policies (see, e.g., Epstein and Walker 1995, 538; Wasby 1993, 205–6). Why has this method of docket management not been subject to more systematic investigation? The answer, we think, is that this practice—long exercised, widely acknowledged, and (until now) not subject to dispute—has not yet come to occupy a place in our theoretical notions of how the Supreme Court establishes an agenda (cf. Pacelle 1991, Ulmer 1982). In other words, many students of the Court are still inclined to think of the justices' agenda building as a discrete process, a dichotomous choice model in which petitions are either accepted or rejected. So it is scarcely surprising that, despite the frequency with which it appears to occur, those who study the Court's decision making might be apt to view issue expansion as entirely idiosyncratic, orthogonal to any system of decision making.

Hence, the challenge that Epstein, Segal, and Johnson mount to our original analysis reflects an understandable reluctance to view issue fluidity within the framework of the justices' process of agenda formation. They believe, instead, that the Supreme Court is governed by a rule that disfavors deciding questions not presented. Unfortunately, the strength of their predilection substantially controls the development of a research design which, though carefully constructed and well executed, leads inexorably to a predetermined conclusion. To fortify this assessment, it is essential to examine the built-in biases of their coding and to compare their results to our own measures.

TESTING OUR CRITICS' MODEL

The track record of research on issue fluidity is not particulary lengthy. As a result, agreement on what constitutes issue fluidity and how best to measure it is not widespread. One way to accentuate the differences between our approach and that of our critics would be to select a case and illustrate the operation of our respective coding rules. In the absence of agreement about which decisions fall under the heading of issue expansion, however, selecting any one case leaves us—and Epstein, Segal, and Johnson, for that matter—open to the charge that it was chosen selectively as a means of playing up the strengths of one strategy while stressing the weaknesses of the other. Seeking to eliminate the potential for such bias, we ask whether there exists some case that scholars uniformly agree illustrates issue expansion by the Supreme Court. Is there some documented case of issue expansion, independent of our original data, on which there is universal agreement? The most likely candidate in this category is *Mapp* v. *Ohio* (1961).

The story of this case is easily summarized.[5] In May 1957, members of the Cleveland police arrived at the home of Dollree Mapp, a woman suspected of harboring a bombing suspect. Over Ms. Mapp's objections, the police, apparently without a warrant, forcibly entered her home and conducted a wide-ranging search, the fruits of which included various books and pictures that the police judged to be obscene. With this evidence, Ms. Mapp was subsequently convicted under an Ohio law that forbade the possession of obscene materials. In the U.S. Supreme Court, the parties focused largely on the constitutionality of that state law, but when the Court announced its opinion, it became clear the justices had eschewed that issue in favor of applying the exclusionary rule to the states. Previously, in *Wolf* v. *Colorado* (1949), the justices had declared that the states were bound by the Fourth Amendment's guarantee of freedom from unreasonable searches and seizures, but it had left the principal federal enforcement mechanism—the exclusionary rule—to the states' discretion. In *Mapp*, however, the Court abandoned the *Wolf* decision, holding that illegally obtained evidence could not be used in state criminal trials.

Neither Ms. Mapp nor the state of Ohio had asked the justices to address this issue. Why, then, did the Court elect to forge such an important ruling when the parties had not presented that question? The answer is that the American Civil Liberties Union, acting as an amicus curiae, served to refocus the central concern of the case by inviting the justices to consider "the issue of whether evidence obtained in an illegal search and seizure can constitutionally be used in a State criminal proceeding" (*Mapp* v. *Ohio*, Brief of the American Civil Liberties Union 1961, 20). The justices, stimulated by a friend of the Court, decided to transform the case by injecting a new issue into their plenary agenda.

Political scientists and legal scholars have repeatedly cited *Mapp* as a case representative of issue fluidity (Adamany 1991, Carp and Stidham 1996, Cortner 1981, Hall 1992, Murphy and Pritchett 1986, Pacelle 1991, Ulmer 1982). Doubtless the justices were equally aware of it, as evidenced by Justice Stewart's letter to Justice Clark, the author of the Court's opinion, admonishing him for crafting a ruling on a point not raised by the parties (Dorin 1982), and subsequent cases only confirm Justice Stewart's view (see, e.g., *Andrews* v. *Louisville & Nashville Railroad Co.* 1972; *Gilmer* v. *Interstate/Johnson Lane Corp.* 1991). In fact, it is (quite literally) a textbook example which, to quote one of our critics, "presents an excellent illustration of the effect amicus curiae briefs can have on the justices" (Epstein and Walker 1995, 538). Given such consistent accounts, we find it hard to imagine a case less likely to spark debate. Fortunately, it meets our most important test; identifying it as a "classic" case, Epstein, Segal, and Johnson agree with our assessment. With such unanimity, *Mapp* can serve as an appropriate palette upon which the two competing coding schemes can readily be compared.

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[5] Our account of *Mapp* v. *Ohio* (1961) is based upon the opinions filed by the justices in that case, as well as the parties' briefs, reprinted in microform, among the *U.S. Supreme Court Records and Briefs*, Washington, DC: Microcard Editions.

How would *Mapp* v. *Ohio* be coded under our mode of data collection? We use an approach that has been profitably employed by other scholars investigating similar research questions; like Spriggs and Wahlbeck's (1995) study of the informational role of briefs amicus curiae, for example, our coding involves a comparison of the summary statements at the outset of the briefs against the Court's authoritative conclusions that are highlighted in the opinions' syllabi.[6] Under the heading of "Questions Presented," parties are required to devote an introductory portion of their briefs to the substantive legal problems posed. So far as we know, this is a credible indicator of what is and is not at issue in a case. After all, based on this information alone, clerks and justices alike make judgments about whether a case merits the Court's attention (Perry 1991).

Having identified the legal issues at stake, one then examines the Court's syllabus to determine if "we know more than we would expect to know based upon the questions presented"; that is, did the justices "rule on an issue completely unique to a case" or "answer a question presented at a level of generalization greater than is necessary to cover that question" (McGuire and Palmer 1995, 700)? Note that, while arguably formalistic, this is a highly reliable classification procedure; relying upon the summaries in both the briefs (i.e., the questions presented) and the opinions (i.e., the syllabi) greatly reduces the likelihood of disagreement among coders. Plumbing the depths of either the merits briefs or the Court's opinion to determine whether an issue genuinely was presented or addressed, by contrast, introduces enormous potential for subjective error.

To illustrate these coding rules, *Mapp* easily serves as a case in point. In their brief on the merits, the attorneys for Ms. Mapp included the following question: "Did the conduct of the police in procuring the books, papers and pictures placed in evidence by the prosecution violate Amendment IV . . . of the United States Constitution?" (*Mapp* v. *Ohio*, Brief of Appellants 1961, i). Since the Supreme Court had previously ruled that the states were bound by the Fourth Amendment, it easily could have disposed of this question by determining whether the Cleveland police had exceeded the constitutional bar on unreasonable searches and seizures. The Court concluded that they had; in reversing Ms. Mapp's conviction, however, the justices also ruled that "all evidence obtained by searches and seizures in violation of the Federal Constitution is inadmissible in a criminal trial in a state court" (p. 643). In other words, the Court not only answered the question presented by concluding that the police conduct violated the Fourth Amendment, but also reached well beyond that question by ruling, as a consequence, the evidence could not be used in a state trial. Had it been in our original data, *Mapp* v. *Ohio* would have been counted as a case of issue expansion.

Asserting that this standard is flawed, Epstein, Segal, and Johnson craft a different method, one that they claim provides a more appropriate measure of issue expansion. Its significant departure from the coding of McGuire and Palmer is threefold. First, they "begin by comparing points raised in the syllabi with the questions listed in the parties' briefs" (p. 848). That is, rather than identify the questions posed by the parties and then determine if the Court transcended those questions, they identify the Court's ruling and then ask whether it can be interpreted as fitting the questions presented. Second, in their search for issues, they explore the full text of the parties' briefs, since "if an issue is raised in the body of a brief (even if it is not a 'Question Presented'), we can hardly say that the Court is engaging in issue creation" (p. 848). Third, consistent with their assertion that the doctrine of *sua sponte* is a norm that governs the Court's decision making, our critics claim that "it tends to generate informal sanctions from other justices when it is not followed" (p. 845). So, when this norm is violated by a majority of the Court, other justices—typically in their dissenting or concurring opinions—formally take exception to the practice.

It is certainly true that our critics' standards are, on the face of it, objective and, we readily concede, intuitively appealing, but reasonable and impartial coding rules by no means guarantee an accurate barometer of political reality (see, e.g., Casper 1976). It is instructive, therefore, to examine the method of Epstein, Segal, and Johnson in application. We test each of their alternative coding rules in turn.

The Search for Questions

When the justices consider briefs on the merits, the "statement of any question presented [is] deemed to comprise every subsidiary question fairly included therein."[7] This rule of the Court, which obviously anticipates that questions will be phrased so as to embrace all ancillary issues, ensures that questions will be presented at their greatest level of abstraction. Drawing considerable attention to the requirement that questions also be "concise" and phrased "without unnecessary detail," Epstein and her colleagues misread this rule and imply that a succinct statement, by definition, cannot be broad in scope (p. 846). The flaw with this reasoning is obvious

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[6] For a detailed description of these coding rules, see McGuire and Palmer (1995, 699–700).

[7] See Rule 14 of the Rules of the Supreme Court of the United States, reprinted in Stern and others (1993, 887–928).

TABLE 1. Application of Epstein, Segal, and Johnson's Coding Rules to *Mapp* v. *Ohio*

Ruling of the Supreme Court: "All evidence obtained by searches and seizures in violation of the Federal Constitution is inadmissible in a criminal trial in a state court."

Coding Rule	Issue Expansion?
Does the ruling cover the Question Presented?	
(Yes) "Did the conduct of the police in procuring the books, papers and pictures placed in evidence by the prosecution violate Amendment IV of the United States Constitution?"	No
Is the issue in the body of the brief?	
(Yes) "The evidence introduced in the trial . . . was procured contrary to the provisions and in violation of appellant's constitutional rights under Amendment IV . . . of the United States Constitution . . . and should not have been received in evidence" (Appellant's brief, p. 26).	No
(Yes) "Under the judicial rules of evidence prevailing in the courts of the State of Ohio, the trial court in this case and the appellate courts on review had every right to rely upon the authority of the *Lindway* case . . . in holding that the criminal evidence obtained in this search was competent and admissible on the trial" (Appellee's brief, p. 9).	No
(Yes) "To argue . . . that a different rule of evidence or a modification of the non-exclusionary rule of evidence adopted by the Courts of the State of Ohio should apply to the instant case is an attempt to open the door to arbitrary application of a judicially created rule of evidence in Ohio" (Appellee's brief, pp. 10–1).	No
Do opinions verify that a question not raised was decided?	
(No) "From the Court's statement of the case one would gather that the central . . . issue on this appeal is whether illegally state-seized evidence is Constitutionally admissible in a state prosecution . . . [A]lthough that question was indeed raised here and below among appellant's subordinate points, the new and pivotal issue brought to the Court by this appeal is whether . . . making criminal the mere knowing possession or control of obscene material . . . is consistent with the rights of free thought and expression" (*Mapp* v. *Ohio*, pp. 673–5).	No
(No) "The occasion which the Court has taken here is in the context of a case where the question was . . . argued only extremely tangentially. . . . I would think that our obligation to the States . . . would demand that we seek that aid which adequate briefing and argument lends to the determination of an important issue. . . . Thus, if the Court were bent on reconsidering *Wolf*, I think that there would soon have presented itself an appropriate opportunity in which we could have had the benefit of full briefing and argument. . . . [A]t the very least, the present case should have been set down for reargument, in view of the inadequate briefing and argument we have received on the *Wolf* point" (*Mapp* v. *Ohio*, pp. 676–7).	No

enough; that the Court expects *brevity* says nothing about the relevant degree of *specificity*.[8]

Traditionally, the Court has sought to limit the scope of its decisions (*Ashwander* v. *Tennessee Valley Authority* 1936). So, assuming that a question is presented—concisely or tediously—in its most general terms, it is fair to say that a ruling whose breadth exceeds the boundaries of that question reflects a degree of issue expansion (Ulmer 1979, 1982). Our original scheme of data collection sought to take this into account. We began with the questions as posed by the parties in their briefs and then asked whether any of the Court's answers to those questions were more sweeping than necessary.

Through a subtle but significant change in coding, illustrated in Table 1, Epstein, Segal, and Johnson reverse this process. With the Court's ruling in hand, they ask whether it can be understood as applying to the issues articulated in the briefs. Stated differently, instead of beginning with "questions" and then looking for "answers," they begin with the answers and seek to apply them to the questions. Testing this rationale in *Mapp* v. *Ohio* reveals that the distinction is scarcely trivial. By their standards, one begins with the justices' holding—the evidence illegally obtained from Ms. Mapp is inadmissible in a state criminal trial—and then asks if it applies to the question of whether the police search violated the Fourth Amendment. Does the ruling cover the question? It certainly does; the justices ruled that the evidence was illegally obtained, and that is precisely the issue the parties presented. That, as a part of the same ruling, the majority also held that such evidence must be excluded from state criminal courts is irrelevant under the coding rules of our critics. Following their guidelines, *Mapp* would not qualify as a case of issue expansion. In fact, had it been in our original data set, it would have been chalked up as an error on our part.

This deficiency in the method of Epstein, Segal, and Johnson is readily revealed by cases in our own data. In *Duckworth* v. *Eagan* (1989), for example, the justices confronted the question of whether, under *Miranda* v. *Arizona* (1966), it is sufficient to inform a suspect that an attorney will be appointed "if and when you go to court."

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[8] Their discovery that, on average, there are more points highlighted in an opinion's syllabus than there are questions presented in a case is quite meaningless unless one knows something about the substantive content of both. One highly plausible interpretation of their finding is that the syllabi of the Court's opinions are simply more detailed than the questions presented in the briefs on the merits.

The specific question posed by the state in its brief was "whether the Seventh Circuit's ... application of *Miranda*, prohibiting the use of objectionable 'magic words,' in advance of rights, is in conflict with the decisions of this Court" (*Duckworth* v. *Eagan*, Brief for Petitioners 1988, i). Concluding that "*Miranda* warnings need not be given in the exact form described in *Miranda* but simply must reasonably convey to a suspect his rights" (p. 195), the Supreme Court indeed responded to this issue but at a level much broader than the parties contemplated. After all, the state only asked about conditioning the availability of a lawyer on some future event; no one asked the Court the more sweeping question of whether reasonably stating one's *Miranda* rights was sufficient. Clearly, the justices provided a good deal more than they were asked, but adherents to the model of Epstein and colleagues must conclude that this is not the case: The ruling covered the question presented. That the ruling covered a good deal more—consider the myriad *other* ways in which *Miranda* warnings might be varied and still reasonably convey rights—is of no consequence.

Likewise, in *DeShaney* v. *Winnebago* (1989), the Court examined the obligations of county social welfare services when a child is severely beaten. The question, as presented by the parties, focused on the narrow concern over whether the state has an obligation to protect a child from parental abuse, once it establishes a program of child protection. Nevertheless, the members of the Court went well beyond the boundaries of that question, holding that "the Due Process Clause [of the Fourteenth Amendment] imposes no duty on the State to provide members of the general public with adequate protective services" (p. 189). So broad a question as whether a state has an affirmative obligation to provide adequate protection was never posed.

Because of the way Epstein, Segal, and Johnson collect their data—going from opinion to briefs, rather than from briefs to opinions—they carefully avoid the Supreme Court's issue expansion in cases such as these. Even *Roe* v. *Wade* (1973), our initial illustration of issue expansion and a case spotlighted as such by one of the critics, does not pass their standard.[9] According to Epstein, Segal, and Johnson, these cases do not qualify as issue expansion. It is not hard to see why; they would never qualify, nor would any other comparable case, since the more limited question will always be included within the more expansive holding.

Exploring Briefs on the Merits

Leaving aside their initial flaw in method, our critics proceed in their replication of our original analysis in a manner quite comparable to ours. We both begin with information from two sources, briefs on the merits and opinions of the Supreme Court, each of which have specific portions that conveniently summarize the legal issues involved in a case. From the Court's opinion, both we and they rely upon the syllabus, the brief abstract that presents the facts of the case, the proceedings in the lower courts, the majority's principal holdings, and so on. Using this synopsis as a means of measuring what the Court did (and did not) conclude struck us as particularly appropriate "because the syllabus underscores the central elements of the Court's decisions.... These summaries are also generally unadorned by distracting dicta" (McGuire and Palmer 1995, 700). Epstein, Segal, and Johnson share these instincts, noting that "they provide reliable roadmaps to Court decisions. In other words, while two independent scholars, examining the contents of a decision might disagree over whether the opinion writer raised a particular issue, no such dissentience can occur over the points listed in the syllabus" (p. 848).

The analogous roadmap for the briefs on the merits is, of course, the section appearing under the heading "Questions Presented." It is here, as in an opinion, that there is an effective outline of a case's essentials; obviously, just as focusing upon syllabi reduces the probability of discrepancy, so, too, does reliance on the "Questions Presented" drastically minimize debate over what legal questions are at issue in a case. Given that a set of merits briefs can routinely reach 100 pages, restricting attention to these headlines is especially important; two independent scholars laboring through the full text of the briefs could easily reach different conclusions about what issues the parties brought before the Court.

This logic would seem to apply with equal force for briefs as well as opinions, but when our critics are unable to locate syllabi points within the "Questions Presented"—which, more often than not, they are unable to do—this logic is cast away. Despite the disagreements that their judgments might well engender, Epstein and her coauthors mount an expedition through the full text of the merits briefs to see if a question can be found which corresponds to the Court's answer. They do not explain how it was determined that one of the primary points of the Court's opinion could be traced to the parties' filings. Unfortunately, their account of their coding procedures is frustratingly vague on this issue, noting only that questions were coded as present if they could be "located" in the "balance of the brief." Even this subjective standard does not suffice in some cases, and their solution is, in effect, to exclude those cases from consideration.[10]

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[9] Despite Epstein and Kobylka's (1992) claim to the contrary, Justice Blackmun's trimester framework, which delineates the interests of the state throughout the term of pregnancy, clearly covers the question, raised by the state, regarding the circumstances under which the state can outlaw abortion: "Whether the state of Texas has a legitimate interest in preventing abortion except under the limited exception of 'an abortion procured or attempted by medical advice for the purpose of saving the life of the mother' " (*Roe* v. *Wade*, Brief for Appellants 1973, 7).

[10] When some of the major holdings in the data speak to different aspects of the Supreme Court's jurisdiction and the parties' briefs provide no trace of these questions, their answer is to exempt them from their requirements under the doctrine of "reverse *sua sponte*," a rule apparently of their own fashioning, by which unasked questions of jurisdiction and justiciability can be answered by the justices at their discretion. Their interpretation is not widely held. "Jurisdictional issues," according to the standard manual on Supreme Court practice, "can always be considered, even sua sponte" (Stern et al. 1993, 346); in other words, parties are entitled, as a matter of course, to present

Aside from its patent problems of reliability, whether our critics have adopted a method that accurately captures how the Court determines what issues are presented in a case is a relevant concern about which they say nothing; such an omission is especially odd in an article otherwise devoted to emphasizing the Supreme Court's adherence to internal norms of decision making. Citing no authority, they merely assert that, if an issue can be "located" within the text of a merits brief, the issue is properly before the Court and therefore cannot be decided *sua sponte*. Is this an outlook shared by the justices? In a word, no. As the members of the Court have frequently emphasized, "the fact that the parties devoted a portion of their merits briefs to [an] issue does not bring that question properly before us" (*Izumi Seimitsu* v. *U.S. Philips Corp*. 1993, 427; see also *Radzanower* v. *Touche Ross & Co*. 1976, 151).[11] As we have argued, the justices remain quite free to manipulate this condition by elevating an issue not formally before them onto the plenary agenda.

Again, *Mapp* provides ample evidence of this shortcoming in our critics' model. The evidence from Table 1 suggests that the argument over the applicability of the exclusionary rule was visited not by one but by both parties. Counsel for Ms. Mapp explicitly maintained that the tainted evidence should not have been admitted at her trial, and the state, with equal clarity, argued that Ohio's decision to allow such evidence should not be disturbed by a mandate of the federal courts. Still, any one of these statements within the briefs, by the standards of our critics, would be prima facie evidence that the issue of binding the states to the exclusionary rule was a question formally presented in *Mapp* v. *Ohio*.

All the more glaringly, this problem dogs their reanalysis of our original data. In *Argentine Republic* v. *Amerada Hess Shipping Corp*. (1989), for example, the justices were asked to interpret federal court jurisdiction over a foreign state under the Foreign Sovereign Immunities Act of 1976, after a cargo ship owned by commercial interests in the United States was attacked by Argentina during its conflict with Great Britain over the Falkland Islands. In one of its principal holdings in the case, the Supreme Court concluded that none of several international agreements—the Geneva Convention on the High Seas, the Pan American Maritime Neutrality Convention, and the Treaty of Friendship, Commerce and Navigation—created exceptions to this law. According to Epstein, Segal, and Johnson, the issue of these exceptions is presented in the following sentence in the respondent's brief:

> The right of an innocent neutral shipowner to seek, and receive, compensation for the illegal destruction of its ship and cargo on the high seas has been recognized for centuries under customary international law. Story, Notes on the Principles and Practices of Prize Courts (Thomas Pratt, ed; London, 1854); Argentine Provisional Regulation for Privateering of 1817; The London Naval Conference of 1909; Pan-American Convention Relating to Maritime Neutrality, Feb. 20, 1928, 47 Stat. 1989; Geneva Convention on the High Seas, Apr. 29, 1958, 13 U.S.T. 2312 (*Argentine Republic* v. *Amerada Hess Shipping Corp*., Brief for the Respondents 1989, 16).

Obviously, this is an assertion of a right to compensation, but whether this is a claim that certain international agreements establish exceptions to the Foreign Sovereign Immunities Act is far less clear. As to the dictates of the Pan American and Geneva Conventions, this language hardly constitutes substantive interpretation. Moreover, that their coding judgments "are clear enough to be beyond any controversy" (p. 848) is a bit of a stretch, since one of the three agreements in the Court's ruling—the Treaty of Friendship, Commerce and Navigation—is mentioned nowhere on this page of the brief.

Nothing here approaches the specificity and directness of the arguments found in the parties' briefs in *Mapp*, a case in which, even our critics concede, the issue decided was not presented by the parties. Still, this is precisely the kind of slender reed upon which Epstein, Segal, and Johnson hang their argument.[12] Cases such as these, others of which we easily could illustrate from our data, reveal the kind of heroic interpretations that can be brought to bear when one abandons the "Questions Presented" for the subjective uncertainties of the "balance of the briefs."

Confirmation from the Court

The most compelling criticism of our analysis is the assertion by Epstein, Segal, and Johnson that, when an issue not presented has been decided, the members of the Court provide confirmatory evidence in their written opinions. In their view, the justices typically will take their colleagues to task, most likely in their concurring or dissenting opinions, for violating the doctrine of *sua sponte*. By that benchmark, one thus reliably can find cases in which the transformation of issues has taken place. The upshot of their argument is that, if the cases we identify provide no such evidence—no statements within the various opinions filed indicating the Court answered a question not raised—then we must be wrong. There is much to commend this logic, since the justices are surely as qualified as anyone to judge whether an

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formally questions of jurisdiction, and, in the absence of these, the justices are free to take up such questions on their own. When this troublesome group of cases arises, however, Epstein, Segal, and Johnson simply declare that category of legal concerns to be off limits. Thus inoculated, these cases are impervious to claims of issue transformation. Although obviously unnecessary in light of their argument, our critics also add that the standard statement as to how the Court's jurisdiction is established—a bill of particulars mandatory in every case—amounts to a "Question Presented." This obligatory bit of recitation, however, is merely " 'boilerplate.' . . . This jurisdictional section is for the benefit of the Clerk's Office and the law clerks" (Baker 1984, 615).

[11] This holds with particular force when parties devote portions of their briefs to issues not passed upon by the lower courts. In such cases, "this is the rule the Court normally applies: 'We do not reach the constitutional questions not raised by the parties. . . . *The fact that the issue was mentioned in argument does not bring the question properly before us*' " (Stern et al. 1993, 344, emphasis added).

[12] In fact, in our own attempts to retrace the steps of our critics, we sometimes found it difficult, even by the broadest stretch, to interpret a ruling as applying to the relevant passages of the merits briefs (see, e.g., *Coit Independence Joint Venture* v. *FSLIC* 1989).

TABLE 2. Cases with Written Opinions Verifying Issue Expansion, 1951–95

Years	Number of cases
1951–55	4
1956–60	6
1961–65	7
1966–70	6
1971–75	13
1976–80	25
1981–85	22
1986–90	22
1991–95	12
TOTAL	117

Note: See Appendix for list of cases.

opinion reaches beyond the boundaries of a case and resolves unasked questions.

Epstein and her colleagues caution, however, that such cases should be few and far between; based on their convictions about the power of the doctrine of *sua sponte*, they assert that one should observe only the most sporadic deviations from the norm. To test their beliefs, we conducted a search of the *U.S. Reports* over the last 45 years to determine the frequency with which the justices filed such opinions. Drawing from a list of key words and phrases we mechanically searched the Court's written opinions for evidence of the brand of informal sanctions our critics envision.[13]

It bears emphasizing that our search was limited to concurring and dissenting opinions, since it is in those that members of the Court raise these objections. One contingency our critics regard as too remote even to contemplate is that the justices may unanimously decide to transform the issues in a case. Since violations of the *sua sponte* doctrine invariably draw criticism from those justices unwilling to abide such aberrant behavior, unanimous cases, by definition, must be excluded. This assumption allows Epstein and colleagues to eliminate a vast number of potential candidates. So much for *Marbury* v. *Madison*.

Despite this limitation, we find little support for the claim that the expansion of issues occurs in a trivial number of cases. The results presented in Table 2 indicate that, while such transformations were intermittent during the Warren Court, over the last 25 years the justices have been quite willing to incur the wrath of their colleagues by manipulating the issues on their plenary agenda. Furthermore, unlike searches for decisions pertaining to broad issue areas—such as search and seizure or death penalty cases, which can be readily distinguished with a few key words—it is virtually impossible to specify all the enormous variations in language that the justices might use to indicate a change in issues. The implication is obvious enough: The data in Table 2 are most assuredly an underestimate. Yet, even as a conservative figure, the incidence of issue expansion uncovered here is, in proportional terms, comparable to the rates at which the Court overturns precedent, invalidates federal legislation, and grants certiorari, "infrequent" decisions which many political scientists regard as systematic and explainable (see, e.g., Caldeira and McCrone 1982, Funston 1975, Perry 1991, Segal and Spaeth 1993, 319–20, 323–4).

What kind of language from these opinions serves to validate issue fluidity? The following examples convey their general flavor.

> All of those persons in Illinois who may have followed the progress of this case will, I expect, experience no little surprise at the Court's opinion handed down today. Stanley will undoubtedly be surprised to find that he has prevailed on an issue never advanced by him. The judges who dealt with this case in the state courts will be surprised to find their decisions overturned on a ground they never considered (*Stanley* v. *Illinois* 1972, 662).
>
> The Court today decides a question the parties did not present, brief, or argue. By so doing, the Court rules without the benefit of "that concrete adverseness which sharpens the presentation of issues upon which the court so largely depends for illumination of difficult constitutional questions" (*State Land Board* v. *Corvallis Sand & Gravel Co.* 1977, 384–5).
>
> I . . . cannot join the Court's opinion because it reaches out to decide a wholly distinct issue not presented and not capable of being treated fairly without further development of a factual record. . . . This question was not presented in the certiorari petition and not a single word is devoted to it in the briefs (*United States* v. *Sharpe* 1985, 700).
>
> This Court inappropriately reaches out to address two *Miranda* issues not raised by the prosecutor in his petition for certiorari (*Colorado* v. *Connelly* 1986, 183–4).
>
> This case could easily be decided within the contours of established First Amendment law. . . . Instead, "find[ing] it unnecessary" to consider the questions upon which we granted review, the Court holds the ordinance facially unconstitutional on a ground that was never presented to the Minnesota Supreme Court, a ground that has not been briefed by the parties before this Court (*R.A.V.* v. *St. Paul* 1992, 397–8).
>
> The Court holds that Amtrak is a Government entity and therefore all of its actions are subject to constitutional challenge. Lebron, however, expressly disavowed this argument below, and consideration of this broad and unexpected question is precluded because it was not presented in the petition for certiorari. The question on which we granted certiorari is narrower (*Lebron* v. *National Railroad Passenger Corp*. 1995, 975).

The justices' concerns or complaints about rulings that are overly broad or that address issues not found among the "Questions Presented" correspond quite favorably with our original coding rules. These samples make clear that at least some members of the Court believed a decision exceeded its proper scope and reached questions not formally presented.

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[13] Our electronic search was conducted on the Law Office Information System's CD-ROM version of the *U.S. Reports* for cases decided from the Terms of the Court, 1951 to 1995. The key words and phrases for our search were the following: advisory opinion, beyond what is necessary, did not brief, did not raise, fully developed record, neither raised, not among the questions presented, not argued, not asked, not briefed, not litigated below, not presented, not raised, not the question presented, Rule 14.1, Rule 21.1, Rule 23.1, Rule 24.1, *sua sponte*, without a full briefing, without argument, without briefing, and without the benefit of argument.

Whatever conclusions one may draw about how often the justices have disparaged the expansion of issues, the more penetrating questions is whether, by this yardstick, *Mapp* v. *Ohio* qualifies as a legitimate case. The information in Table 1 presents what definitely appears to be precisely the same manner of remarks; writing in dissent, Justice Harlan protested the Court's decision to use that case to examine a question not presented. A careful review of our critics' coding procedures, however, quickly reveals that Harlan's dissent does not suffice. True enough, he begins by claiming that the issue of admissibility in state courts of evidence obtained by illegal searches was not properly presented, but according to our critics, any concession that the question was raised in the litigation—however remotely—invalidates his claim. Harlan's opinion therefore fails to qualify because it maintains not that the question was never raised but that it was not presented sufficiently; he admits that, although it was "argued only extremely tangentially," the applicability of the exclusionary rule was indeed an issue in the case. Likewise, he is dismayed the decision had been reached without "the benefit of full briefing." As Epstein, Segal, and Johnson explain, such language "suggests that the issue was in fact briefed, and that is the case.... Thus, under the circumstances, it would be rather difficult to claim that issue was not part of the legal record" (p. 852). As we have already noted, the question of binding the states to the exclusionary rule can be "located" within the briefs. Once again, *Mapp* v. *Ohio* cannot overcome the hurdles put in place by Epstein, Segal, and Johnson.

Notwithstanding our critics' dismissal of *Mapp*, their more general suggestion that the Court's opinions can serve to flag bona fide cases of issue expansion remains highly plausible. Accordingly, the acid test is whether any of the cases in our original data set contained such opinions, and, if so, whether those opinions were filed in the same cases in which we claimed that the plenary agenda had been expanded. It turns out that, of the cases identified by our search, eight were decided during the 1988 term; more important, six of those eight were precisely ones we had labeled as cases of issue transformation.[14] In other words, both we and the justices—completely independent of each other—came to substantially the same conclusions.[15]

The following are the relevant excerpts from those opinions.

> I ... do not join the latter part of the Court's opinion to the effect that none of the FSIA's exceptions to foreign sovereign immunity apply in this case. As the majority notes, the Court of Appeals did not decide this question.... Moreover, the question was not among those presented to this Court in the petition for certiorari, did not receive full briefing, and is not necessary to the disposition of the case. Accordingly, I believe it inappropriate to decide here, in the first instance, whether any exceptions to the FSIA apply in this case (*Argentine Republic* v. *Amerada Hess Shipping Corp.* 1989, 443).

> I join Parts I, II, and III of the Court's opinion. I refrain from joining Part IV and thus concur only in the judgment. My concern with Part IV is that it seems to me to amount to only an advisory opinion on what the Bank Board may do, based on a surmise of what the Bank Board might someday conclude it must do in order to liquidate "in an orderly manner" (*Coit Independence Joint Venture* v. *FSLIC* 1989, 588).

> I do not join the Court's assessment of this case under the Equal Protection Clause. Although the equal protection issue received nominal attention in the trial court.... it was neither reviewed by the Texas Court of Appeals nor briefed before us (*Dallas* v. *Stanglin* 1989, 29).

> It may well be, as the Court decides ... that the Due Process Clause ... creates no general right to basic governmental services. That, however, is not the question presented here; indeed, that question was not raised in the complaint, urged on appeal, presented in the petition for certiorari, or addressed in the briefs on the merits. No one, in short, has asked the Court to proclaim that, as a general matter, the Constitution safeguards positive as well as negative liberties (*DeShaney* v. *Winnebago County Social Services Dept.* 1989, 203–4).

> The Court, I fear, because of what it regards as the investment of time in having this case argued and briefed, is strong-arming the [jurisdictional] concept.... We have jurisdiction ... only if there is a "final judgment" by the "highest court of a State" in which a decision could be had (*Duquesne Light Co.* v. *Barasch* 1989, 317).

> In the past, this Court has overruled decisions antagonistic to our Nation's commitment to the ideal of a society in which a person's opportunities do not depend on her race ... and I find it disturbing that the Court has in this case chosen to reconsider, without any request from the parties, a statutory construction so in harmony with that ideal (*Patterson* v. *McLean Credit Union* 1989, 191).

Among other things, they inveigh against deviations from the questions presented as well as the absence of adequate briefing and argument. These opinions, we think, do not invite wildly varying interpretations; indeed, the tenor of this language is difficult to distinguish from the companion examples we have already sketched. As such, they strike us as illustrations of a majority's capacity to remold a case to suit its purposes, even at the cost of their colleagues' objection. We are convinced, therefore, that this feature of our critics' coding is a useful and reliable method for helping to pinpoint legitimate cases of issue expansion; since it successfully unearths many of the same cases we had placed in that category, we can hardly conclude otherwise.

In the analysis of our critics, however, our data fare no better than *Mapp* v. *Ohio*. Confronted with this highly confirmatory evidence, Epstein, Segal, and Johnson cast it all away with the following rationale: The justices are wrong. In a series of footnotes to their appendix (see p. 852), they dismiss all of these "informal sanctions," declaring that they instead reflect a justice's ill-advised misstatement, poor judgment, or misreading of the briefs.[16] So, in some instances—at least those cases within our data, certainly—the members of the Court cannot be trusted to provide the necessary corroborative evidence. According to this view, not only we but also the justices are wrong and (coincidentally) about the same cases.[17]

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[14] Moreover, one of the remaining two, which protested issue expansion by a mere plurality of the Court, could not have qualified by our standards. By our original coding rules, a question could qualify as expanded only if the treatment of that issue was found in an opinion (or one of its component parts) to which a majority of justices subscribed.

[15] In one case, even the majority opinion provided additional confirmation, noting that "although the parties have not discussed it, we must first inquire into our jurisdiction to decide this case" (*Duquesne Light Co.* v. *Barasch* 1989, 306).

By each of our critics' standards, then, our data fail to measure up: Broad rulings cover narrow questions, issues lurk within the briefs, and the justices create mistaken impressions of issue expansion. The proof of our shortcomings is dubious, however, if the method used to arrive at that judgment is itself flawed. We conclude that it is. Transfixed by the doctrine of *sua sponte* and unwilling to consider issue fluidity as a possible component of the Supreme Court's agenda setting, Epstein, Segal, and Johnson make a serious error of empirical analysis: They allow their predispositions to guide both their appraisal of our contrary findings and the design of their own research; not surprisingly, prior beliefs receive considerable support (see Gould 1996).

In evaluating our original analysis, for example, Epstein and colleagues indict our method on a number of grounds, not the least of which is its use of key summaries within the briefs as a means of calibrating the issues that parties present to the Court. Our critics counsel what they believe to be a more prudent approach, citing as a worthy model recent scholarship by Spriggs and Wahlbeck (1995) on briefs amicus curiae. Ironically, our coding scheme is virtually identical to Spriggs and Wahlbeck's. Still, our design is judged seriously deficient, while this same method employed in a different context produces results with which our critics "do not, and cannot, take issue" (p. 849). It appears that they laud the method when it generates agreeable results and condemn it when it leads to conclusions less to their liking.

Moreover, in developing their own research strategy, our critics establish coding rules that make it difficult, and in some cases impossible, for the Court to expand the range of issues. At the same time, some of those rules are left vague and flexible enough so that subjective judgments favoring their hypothesis are possible. Finally, Epstein and colleagues demand independent confirmation from the Court, but when the justices provide it, it is pronounced undependable, explained away, and banished to an appendix.

In the end, our critics accuse us of not understanding the subtleties of a collegial court bound by important norms that impose significant constraints on the justices' discretion. Supported by a set of alternative tests, Epstein, Segal, and Johnson dismiss our claims of issue expansion. By their tests, we are only too willing to concede, the data often do not support our conclusions. Of course, we should also emphasize that, by those same standards, *Mapp* v. *Ohio*—the classic case of issue expansion, acknowledged by political scientists, legal scholars, the justices, and even our challengers—fails as well. Not only does it fail, it fails utterly and miserably.

THE CASE OF CONSTRUCT VALIDITY

An important question remains: Did our original analysis genuinely measure transformations in the Supreme Court's plenary agenda, or did we assess degrees of adherence to the doctrine of *sua sponte*? Whether a measure accurately taps the property it purports to gauge is a principal question in any research design. One of the more effective ways of resolving the issue of construct validity is through convergent and discriminant procedures; principally, this involves a systematic comparison of different attributes measured by multiple means (Campbell and Fiske 1959). The central assumption is that "correlation coefficients among scores for a given property measured by different instruments should be higher than correlations among different properties measured by similar instruments" (Nachmias and Nachmias 1987, 171). Fortunately, we are provided with just such a scenario.

In this context, we and our critics employ techniques of measurement that are roughly equivalent; with some important differences, both coding systems require one to compare the syllabi from the Court's opinions against the contents of the briefs on the merits. We clearly have conflicting views, however, about what concepts are reflected through this instrumentation.

Quite separate from either of these measures, we have the independent evaluations of the justices themselves. It is obvious from their written opinions that the members of the Court sometimes conclude that a majority has addressed issues not presented. Since the syllabus in a case is prepared by the Clerk of the Court only after all the opinions have been written, the justices certainly are using a different procedure for arriving at their conclusions. Whether those protests are sparked by the majority's agenda transformations or by violations of the Court's norm against deciding issues *sua sponte* remains to be seen; in either case, it is still an independent estimate of a characteristic, calculated by a wholly separate method.

It follows that, if Epstein, Segal, and Johnson are correct, then the correlation between their coding of violations of the Court's norm and the justices' informal sanctions (i.e., the correlation between two different measures of the same attribute) should be greater than the relationship between our critics' results and our original coding of issue expansion (i.e., the correlation between two different attributes measured by a common method). If, however, our coding correlates more highly with the justices than with our challengers, then that would suggest the concept of issue expansion on the Supreme Court is the more meaningful one.

With the data from the 1988 term ($N = 160$), the correlations for these three different measures are easily computed.[18] (Epstein, Segal, and Johnson only replicated our analysis for one category of our variable; since they do not question it, we assume that they agree with our coding when no issue transformation was found.) The results are revealing: The correlation between the judgments of the justices and Epstein, Segal, and Johnson is −0.02, while the relationship between our coding and that of our critics is 0.23. This is exactly the opposite of what one would expect to find if the concept of *sua sponte* had validity. In contrast, at 0.48, the correlation between our coding and the Court's is substantially greater than either of the other two, suggesting that issue fluidity carries the more plausible interpretation. Measured by alternative means, judgments about the nature of transformations in the high court's plenary agenda track one another quite closely indeed.[19]

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[16] Epstein and her coauthors claim to deal exhaustively with such opinions; curiously, they offer no explanation for the opinions in two of these six cases.

[17] It stresses credulity to argue that we were wrong in every case *and* that the justices were wrong in every case *and* that, even though they are substantively similar, each of the justices' errors are explained by idiosyncratic factors *and* that our combined "errors" (i.e., McGuire/Palmer's and the justices') are unrelated. If our critics' analysis were a regression model, it would clearly violate one of its basic assumptions (Berry and Feldman 1985, 73–8).

We hasten to add, however, that because of limited variance the two correlations involving our critics' measure are suspect. Furthermore, in fairness to Epstein, Segal, and Johnson, we cannot be certain how they would have coded the remaining cases. A general survey of the data they do code remains equally telling, nonetheless. Among the 18 cases they examine, their method of analysis uncovered none of the cases in which the justices protested issue expansion. Observing our coding rules, we located them all.

We do not consider our analysis to be the last word on the question of issue expansion. The only claim we make here is that, given the choice between the *sua sponte* doctrine and issue fluidity, ours is the more responsible construction.

CONCLUSION

Deciding whether to transform the issues in a case is, we think, essentially an exercise in agenda setting. The justices continually face the responsibility of determining what cases—and thus what issues—deserve their scarce attention. Ideological considerations play a leading role in this process; so, given the Supreme Court's broad discretionary power, it seems entirely likely that the justices may, from time to time, find it in their interest to remold the issues in a case as a means of realizing their policy goals. Such a position is both consistent with and amply supported by research on the process of agenda building in the high court.

We have revisited our earlier findings on the frequency with which the Court capitalizes upon this aspect of its discretionary authority, and our survey reinforces those initial claims. Casting a wider net, we have also found evidence to suggest that our findings are by no means isolated to the term we first examined. Over the last quarter-century, by our conservative estimate, issue expansion has been a modest but steady feature of the Supreme Court's plenary docket.

No one disputes that there are norms militating against the wholesale pursuit of the justices' policy ambitions, and the norm against deciding issues not presented is clearly among them. Still, these are "limitations not absolute bars to the exercise of Supreme Court power" (Murphy 1964, 29); like all norms, it can be transgressed by the members of the Court, and, when a majority decides to chart a new course in a case, some justices may be apt to decry their colleagues' decision. Why does this occur? Do such outcomes manifest varying degrees of faith in the doctrine of *sua sponte*, or are they, like much of the Court's agenda setting, ideologically motivated?

As a capstone to their argument, Epstein, Segal, and Johnson draw instructive value from *Patterson* v. *McLean Credit Union* (1989), one of the cases in our data set in which the majority, with no request from the parties, decided to entertain the advisability of overruling the precedent established in *Runyon* v. *McCrary* (1976). To certify the force of the *sua sponte* doctrine, they note the fallout from this decision to revisit unilaterally the decision in *Runyon*; justices Blackmun, Brennan, Marshall, and Stevens dissented, voicing their strong disagreement with the majority's decision to expand the questions presented. According to our critics, such sanctions help substantiate the doctrine of *sua sponte*.[20]

One need only inquire as to what these four justices have in common to formulate an alternative hypothesis: They were all members of the original majority in *Runyon*. Given the threat of overturning a precedent they themselves had a hand in crafting, we find it not at all surprising that the original members of the *Runyon* majority would dissent in the face of this transformation of the plenary agenda. The more straightforward explanation is that these justices were simply animated by their own values. There was a time when conscientious scholars believed that the Stoic traditions of legal analysis thoroughly governed the decisions of the Supreme Court and that the policy preferences of the justices played little role in their calculus. We had thought that such a time was long since passed.

APPENDIX

Based on our search of the U.S. Reports (1951–95 terms) the following were identified as cases involving issue expansion. These data serve as the basis for the frequency distribution presented in Table 2.

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[18] Our coding led us to conclude that issue expansion occurred in 18 cases (McGuire and Palmer 1995, 697), but in replicating our analysis we discovered that, because of a missing merits brief, one of those cases should not have been included. Parenthetically, this case, *Fort Wayne Books, Inc.* v. *Indiana* (1989), was one of the three classified as errors by Epstein, Segal, and Johnson. So we claim issue expansion as having occurred in 17 cases. By contrast, our challengers allow that an issue not formally presented was decided in only one case, the "highly explicable exception" of *City of Richmond* v. *J.A. Croson Co.* (1989).

[19] Since issue expansion is being measured by two different methods, this is not a question of intercoder reliability, which measures agreement between coders using the same measurement tool. Even by that standard, however, our agreement with the justices would still be more than acceptable by the protocol of replication (kappa = .47; see Elder, Pavalko, and Clipp 1993).

[20] According to Epstein and colleagues, however, this case still does not qualify as a legitimate instance of issue expansion. The issue of reversing *Runyon* clearly was not raised, but the Court cannot be said to have engaged in issue expansion, say our critics; although it considered this question "without prompting or suggestion," that the Court ordered the parties to brief this issue is sufficient to establish it as a question the parties "asked" the Court to decide.

1 *Albrecht* v. *Herald Co*. 1968. 390 U.S. 145
2 *Andrews* v. *Louisville & Nashville R. Co*. 1972. 406 U.S. 320
3 *Argentine Republic* v. *Amerada Hess Shipping*. 1989. 488 U.S. 428
4 *Arizona Governing Committee* v. *Norris*. 1983. 463 U.S. 1073
5 *Arkansas* v. *Sanders*. 1979. 442 U.S. 753
6 *Bangor Punta Corp*. v. *Chris-Craft Industries, Inc*. 1977. 430 U.S. 1
7 *Batson* v. *Kentucky*. 1986. 476 U.S. 79
8 *Beck* v. *Ohio*. 1964. 379 U.S. 89
9 *Bellotti* v. *Baird*. 1979. 443 U.S. 622
10 *Berry* v. *Doles*. 1978. 438 U.S. 190
11 *Board of Curators, Univ. of Mo*. v. *Horowitz*. 1978. 435 U.S. 78
12 *Brown* v. *Socialist Workers '74 Campaign Comm*. 1982. 459 U.S. 87
13 *California Federal S. & L. Assn*. v. *Guerra*. 1987. 479 U.S. 272
14 *California* v. *United States*. 1978. 438 U.S. 645
15 *Central Bank* v. *First Interstate Bank*. 1994. 114 S.Ct. 1439
16 *Coit Independence Joint Venture* v. *FSLIC*. 1989. 489 U.S. 561
17 *Coker* v. *Georgia*. 1977. 433 U.S. 584
18 *Colorado* v. *Connelly*. 1986. 479 U.S. 157
19 *Cooper* v. *New York*. 1953. 346 U.S. 156
20 *Concrete Pipe & Prods*. v. *Constr. Laborers Trust*. 1993. 113 S.Ct. 2264
21 *Corvallis Sand & Gravel Co*. v. *Oregon Ex Rel. State Land Board*. 1977. 429 U.S. 363
22 *Culombe* v. *Connecticut*. 1961. 367 U.S. 568
23 *Dallas* v. *Stanglin*. 1989. 490 U.S. 19
24 *Darby Drug Co., Inc*. v. *Ives Laboratories, Inc*. 1982. 456 U.S. 844
25 *Deshaney* v. *Winnebago Cty. Soc. Servs. Dept*. 1989. 489 U.S. 189
26 *Duquesne Light Co*. v. *Barasch*. 1989. 488 U.S. 299
27 *East Carroll Parish School Bd*. v. *Marshall*. 1976. 424 U.S. 636
28 *Electrical Workers* v. *Foust*. 1979. 442 U.S. 42
29 *Evans* v. *Newton*. 1966. 382 U.S. 296
30 *Evans* v. *United States*. 1992. 504 U.S. 255
31 *Farmer* v. *Brennan*. 1994. 114 S.Ct. 1970
32 *First Boston Corp*. v. *Chris-Craft Industries, Inc*. 1977. 430 U.S. 1
33 *Florida* v. *Wells*. 1990. 495 U.S. 1
34 *Ford Motor Co*. v. *EEOC*. 1982. 458 U.S. 219
35 *Ford Motor Co*. v. *NLRB*. 1979. 441 U.S. 488
36 *FTC* v. *Fred Meyer, Inc*. 1968. 390 U.S. 341
37 *Fuller* v. *Oregon*. 1974. 417 U.S. 40
38 *Garner* v. *United States*. 1976. 424 U.S. 648
39 *Gravel* v. *United States*. 1972. 408 U.S. 606
40 *Gray* v. *Sanders*. 1963. 372 U.S. 368
41 *Greer* v. *Miller*. 1987. 483 U.S. 756
42 *Grove City College* v. *Bell*. 1984. 465 U.S. 555
43 *Gutierrez De Martinez* v. *Lamagno*. 1995. 115 S.Ct. 2227
44 *Hayes* v. *Florida*. 1985. 470 U.S. 811
45 *Herbert* v. *Lando*. 1979. 441 U.S. 153
46 *Hudson* v. *North Carolina*. 1960. 363 U.S. 697
47 *In Re Gault*. 1967. 387 U.S. 1
48 *In Re Ruffalo*. 1968. 390 U.S. 544
49 *Inwood Laboratories* v. *Ives Laboratories*. 1982. 456 U.S. 844
50 *Lafayette* v. *Louisiana Power & Light Co*. 1978. 435 U.S. 389
51 *Lake Country Estates* v. *Tahoe Planning Agcy*. 1979. 440 U.S. 391
52 *Lebron* v. *National Railroad Passenger Corporation*. 1995. 115 S.Ct. 961
53 *Mackey* v. *Mendoza-Martinez*. 1960. 362 U.S. 384
54 *Mapp* v. *Ohio*. 1961. 367 U.S. 643
55 *Massachusetts Mut. Life Ins. Co*. v. *Russell*. 1985. 473 U.S. 134
56 *May* v. *Anderson*. 1953. 345 U.S. 528
57 *Memphis* v. *Greene*. 1981. 451 U.S. 100
58 *Missouri* v. *Jenkins*. 1995. 115 S.Ct. 2038
59 *McCleskey* v. *Kemp*. 1987. 481 U.S. 279
60 *McCleskey* v. *Zant*. 1991. 499 U.S. 467
61 *New Jersey* v. *T.L.O*. 1985. 469 U.S. 325
62 *New York Gaslight Club, Inc*. v. *Carey*. 1980. 447 U.S. 54
63 *NLRB* v. *Electrical Workers*. 1987. 481 U.S. 573
64 *Nursing Home Pension Fund* v. *Demisay*. 1993. 113 S.Ct. 2252
65 *Oscar Mayer & Co*. v. *Evans*. 1979. 441 U.S. 750
66 *Orr* v. *Orr*. 1979. 440 U.S. 268
67 *Patterson* v. *McLean Credit Union*. 1988. 491 U.S. 164
68 *Pennsylvania* v. *Finley*. 1987. 481 U.S. 551
69 *Pennsylvania* v. *Mimms*. 1977. 434 U.S. 106
70 *Penry* v. *Lynaugh*. 1989. 492 U.S. 302
71 *Piper* v. *Chris-Craft Industries*. 1977. 430 U.S. 1
72 *Quern* v. *Jordan*. 1979. 440 U.S. 332
73 *R.A.V*. v. *St. Paul*. 1992. 505 U.S. 377
74 *Reno* v. *Catholic Soc. Svcs., Inc*. 1993. 113 S.Ct. 2485
75 *Riggins* v. *Nevada*. 1992. 504 U.S. 127
76 *Rose* v. *Lundy*. 1982. 455 U.S. 509
77 *Rose* v. *Rose*. 1987. 481 U.S. 619
78 *Rosenberg* v. *Fleuti*. 1963. 374 U.S. 449
79 *Rushen* v. *Spain*. 1983. 464 U.S. 114
80 *Sangamon Valley Corp*. v. *United States*. 1958. 358 U.S. 49
81 *Shaffer* v. *Heitner*. 1977. 433 U.S. 186
82 *Siegert* v. *Gilley*. 1991. 500 U.S. 226
83 *Southland Corp*. v. *Keating*. 1984. 465 U.S. 1
84 *St. Louis* v. *Praprotnik*. 1988. 485 U.S. 112
85 *Stanley* v. *Illinois*. 1972. 405 U.S. 645
86 *State Land Board* v. *Corvallis Sand & Gravel Co*. 1977. 429 U.S. 363
87 *Stein* v. *New York*. 1953. 346 U.S. 156
88 *Stevens* v. *Marks*. 1966. 383 U.S. 234
89 *Stevens* v. *McCloskey*. 1966. 383 U.S. 234
90 *Sun Oil Co*. v. *Wortman*. 1988. 486 U.S. 717
91 *Sweezy* v. *New Hampshire*. 1957. 354 U.S. 234
92 *Teague* v. *Lane*. 1989. 489 U.S. 288
93 *Thornburg* v. *Gingles*. 1986. 478 U.S. 30
94 *United States* v. *Dixon*. 1993. 113 S.Ct. 2849
95 *United States* v. *Feola*. 1975. 420 U.S. 671
96 *United States* v. *Gravel*. 1972. 408 U.S. 606
97 *United States* v. *Hasting*. 1983. 461 U.S. 499
98 *United States* v. *Hollywood Motor Car Co*. 1982. 458 U.S. 263
99 *United States* v. *Johns*. 1985. 469 U.S. 478
100 *United States* v. *Karo*. 1984. 468 U.S. 705
101 *United States* v. *Kozminski*. 1988. 487 U.S. 931
102 *United States* v. *Leon*. 1984. 468 U.S. 897
103 *United States* v. *Payner*. 1980. 447 U.S. 727
104 *United States* v. *Place*. 1983. 462 U.S. 696
105 *United States* v. *Richardson*. 1974. 418 U.S. 166
106 *United States* v. *Sharpe*. 1985. 470 U.S. 675
107 *United States* v. *Watson*. 1976. 423 U.S. 411
108 *Utah Commission* v. *El Paso Gas Co*. 1969. 395 U.S. 464
109 *Vaca* v. *Sipes*. 1967. 386 U.S. 171
110 *Vlandis* v. *Kline*. 1973. 412 U.S. 441
111 *Washington* v. *Davis*. 1976. 426 U.S. 229
112 *Wayte* v. *United States*. 1985. 470 U.S. 598
113 *Weinberger* v. *Salfi*. 1975. 422 U.S. 749
114 *Western Air Lines* v. *Board of Equalization*. 1987. 480 U.S. 123
115 *Wissner* v. *New York*. 1953. 346 U.S. 156
116 *Wong Sun* v. *United States*. 1963. 371 U.S. 471
117 *Wood* v. *Georgia*. 1981. 450 U.S. 261

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Black Politics at the Crossroads? Or in the Cross-Hairs?

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The Black Progress Question: Explaining the African American Predicament. Stephen Burman. Thousand Oaks, CA: Sage Publications, 1995. 237p. $52.00 cloth, $24.00 paper.

Faded Dreams: The Politics and Economics of Race in America. Martin Carnoy. New York: Cambridge University Press, 1994. 286p. $29.95.

Separate and Unequal: Blacks Americans and the US Federal Government. Desmond King. New York: Oxford University Press, 1995. 352p. $35.00.

Separate City: Black Communities in the Urban South, 1940–1968. Christopher Silver and John V. Moeser. Lexington: The University of Kentucky Press, 1995. 220p. $29.95.

The Bubbling Cauldron: Race, Ethnicity, and the Urban Crisis. Edited by Michael Peter Smith and Joe R. Feagin. Minneapolis: University of Minnesota Press. 359p. $49.95 cloth, $19.95 paper.

We Have No Leaders: African Americans in the Post-Civil Rights Era. Robert C. Smith. Albany: State University of New York Press, 1996. 363p. $46.50 cloth, $16.95 paper.

At the dawn of the twentieth century, W. E. B. Du Bois, in the Forethought to *The Souls of Black Folk* (1903), made his now famous observation on the fundamental political problem then facing the United States: "The problem of the Twentieth Century is the problem of the color line." As we move toward the twenty-first century, his observation, unfortunately, is still correct. Race has been and continues to be a principal thread in the American political fabric, a thread so strong yet simultaneously so fragile that politicians and political parties know that pulling it may win elections. We still hotly debate questions of race and how the political system should respond to the inequalities and discrimination now institutionalized in the process. While Du Bois and his colleagues battled, unsuccessfully, to prevent the disenfranchisement of most African Americans after *Plessy* v. *Ferguson* in 1896, the current struggles are just as significant and just as contentious. The present debate is not only over mechanisms and policies to address the lingering effects of an earlier time, but also over the continuing discriminatory and exclusionary practices present in U.S. society today. We are holding this debate at a time when the polity and the public are particularly antagonistic toward black Americans and toward public policies that are perceived to work to their advantage.

Despite the obvious presence of racial hostility, U.S. society is still reluctant to admit that race plays a significant role in the allocation of resources and benefits from the political system. We approach questions of race and treat them like a family who is hiding the crazy aunt in the attic (or the crazy uncle in the basement). Everyone knows she is there, we feel and hear her presence, but we refuse to admit that her shouts and musings are creating a dysfunctional situation. We choose instead to continue our charade, behaving as if her intrusions have not influenced our behavior toward or relationships with one another.

The "problem of the color line" is a continuing and vexing one. Each book discussed in this review article tackles some aspect of the status of black political progress in the United States. Three broad themes that emerge from these books structure this article. One theme is that the black political condition in the United States is influenced by external political and governmental forces and structures, historical and ongoing. Those structural forces are decisions and actions that occur outside the broader black communities and continue to erect barriers to the incorporation of blacks into the political process. The second theme also involves an external force, but the structures involved are economic rather than political. The third theme is the contribution of internal political debates over strategies to counter these external forces. A subtext of the third theme is the influence of internal structural features (i.e., class divisions) that generate contentious debates over strategies and policy outcomes to overcome the external barriers.

EXTERNAL POLITICAL AND GOVERNMENTAL FORCES: CONTINUING DISPARATE TREATMENT

One issue prominent in current political discourse is the extent to which discrimination and racism still exist in U.S. society and the extent to which they are obstacles to black social and political progress. Some have declared that racism and discrimination are artifacts of a time long past. Nevertheless, the argument goes, even if they do still exist, racism and discrimination are lone acts by individuals against individuals. They are no longer institutionalized in the political and social system, and no effects of discrimination and racism against previous generations of blacks linger today. Examples of this approach to issues of race abound, the most recent being the explanations offered for the rash of black church burnings.

Many individuals have gone to great lengths to attempt to argue that since white churches also have been burned, black churches have been burned for reasons other than racial animus. Even if one were to accept this nonracial explanation, how do we explain the differences in how some insurance companies have treated white and black churches after the fires? Several black

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churches, to their dismay, have found their policies canceled by the very same company that accommodated and reinsured the burned white churches. No matter how much we try to explain race away in one venue, it raises its head in another. The issue of disparate treatment of black Americans in a democratic society is an ongoing predicament.

Desmond King's *Separate and Unequal* makes a significant contribution to our knowledge of the politics of disparate treatment. Through extensive archival research, he documents how, in the eight decades before the passage of the Civil Rights Act of 1964, the federal government contributed to and maintained segregated race relations in the United States. King has produced a noteworthy and well-documented study that, while historical in orientation, raises questions that are no less relevant today. The book traces the reform of the federal Civil Service from a patronage to a merit-based system and shows how the federal government used these reforms as mechanisms to discriminate against blacks. (King's brief historical examination of presidential racial politics from Theodore Roosevelt to Harry Truman is as readable and informative, although less detailed, as Kenneth O'Reilly's *Nixon's Piano: Presidents and Racial Politics from Washington to Clinton* [1995].

The Pendleton Act of 1883 was the first important piece of reform legislation for the U.S. Civil Service. The act established the U.S. Civil Service Commission, giving it the responsibility to design a meritocratic system (i.e., competitive examinations) and to monitor political influences within the Civil Service. Yet, the merit-based process that was supposed to provide neutral criteria for job selection became an instrument used to discriminate against and limit the accessibility of black Americans to employment opportunities in the federal government. In 1914, the Civil Service Commission made photographs mandatory on all application forms, saying the pictures were necessary to prevent impersonation at examinations. After pressure from the National Association for the Advancement of Colored People (NAACP), the commission admitted that it also gave the pictures to the appointing officers to help them in making choices among qualified and certified applicants. A 1928 survey of segregation in government departments by the NAACP reported that many department heads admitted that the requirement of a picture allowed them to eliminate blacks certified as eligible for employment. This practice also allowed for the hiring of blacks into segregated job categories.

The Classification Act of 1923 introduced another mechanism into the selection process that on the surface was neutral but in practice was used to discriminate against black applicants. That new mechanism was the "rule of three." Hiring officials selected a candidate from a list of three certified eligible persons supplied by the Civil Service Commission. Nevertheless, hiring officials could select an individual farther down the list simply by providing reasons for passing over the three highest candidates. If a certified individual was considered three times and not appointed, then the Civil Service Commission removed the individual from the list of three top eligibles.

The NAACP again complained that this supposedly neutral process was, in practice, being used to discriminate against eligible black applicants. The organization cited many instances throughout the federal government of hiring officials bypassing black applicants at the top of the list to hire whites farther down. The result of these practices, King says, among others, was that (1) blacks had difficulties entering civil service; (2) if the federal government hired blacks, they hired them into segregated low-level job categories, despite their education and training (i.e., black lawyers hired as messengers in the Department of Commerce); (3) gaining employment in agency field offices was nearly impossible; and (4) blacks if hired were put in temporary rather than permanent positions and were blocked from moving into permanent openings as they arose.

While the first part of King's book is a general overview of the situation of blacks in the federal government before the 1960s, the later chapters examine the practices in selected agencies. One comes away from these chapters with a knowledge of the extent to which segregation and racism were institutionalized in the federal system. For instance, not only were blacks in civilian federal and military service segregated, but also the government segregated prisoners in the federal prison system as well. King demonstrates how the federal government colluded in the maintenance of segregated race relations in the half-century before the Civil Rights of 1964 and used its authority either to impose or to adapt to segregated race relations in government departments and public policies.

King's conclusions of past bias in the federal system are apparently still pertinent today. He reminds us that in practice individuals can manipulate supposedly neutral policies for discriminatory purposes. The Bureau of Alcohol, Tobacco and Firearms (ATF) recently settled a class action lawsuit filed in 1990 by 241 black agents. Evidence they presented to document long-standing discrimination by the ATF parallels many of King's findings from an earlier time. ATF hired 80% of blacks without Civil Service protection, compared with 15% of whites; they hired in the lowest pay grade possible and often steered blacks (40%) into dangerous undercover jobs compared with whites (25%); and although blacks are only 10% of ATF agents, the bureau disciplines them 50% more frequently than whites (Thomas 1996). Similar situations have been identified and litigated recently in the FBI as well.

Christopher Silver and John V. Moeser's *Separate City: Black Communities in the Urban South, 1940–1968* also examines aspects of disparate treatment of blacks in the political process. Unlike King, however, they focus on local government actions in the urban South. Silver and Moeser use geographical decision making to highlight the political effects of disparate treatment in three southern cities—Richmond, Virginia; Atlanta, Georgia; and Memphis, Tennessee. The use of spatial decisions provides a physical context in which to place political phenomena and creates another perspective from which to view political activity. Unfortunately, few political scientists use this method to study politics, an approach that could inform our conclusions.

The strength of the Silver and Moeser book is the linkage it provides between decisions made about physical space in urban communities and their intended use as mechanisms of economic and political oppression. Several mechanisms examined, such as racial zoning, are not well explored in political science literature. Both Atlanta (1913) and Richmond (1911) passed racial zoning ordinances. City officials assigned a racial designation as either white or black to every city block, based on the racial composition of the neighborhood at the time. The law prohibited new residents of the other race from moving into the neighborhood. The authors show the extent to which segregated housing patterns were the consequence of conscious political decisions in many different arenas to achieve a "separate city" for blacks. Decisions ranging from the placement of public housing to highway construction to business zoning to annexation were all geared toward developing and maintaining segregated residential patterns.

It is within the context of these purposeful segregated residential patterns that Silver and Moeser examine the politics of Atlanta, Richmond, and Memphis from 1940 to 1968. (The last chapter brings the reader up to date on each city). They study the interaction of population growth, social reorganization, and the formation of political leadership in each city. The case studies are interesting and informative. They develop rich and nuanced portraits—the interaction of blacks with Boss Crump's political organization in Memphis; the preeminent role of James Jackson Kilpatrick, then the editor of the *Richmond News Leader* and now a syndicated columnist, in fueling Virginia's massive resistance to school desegregation; and the efforts and influence of John Hope and the faculty at the Atlanta University Center in trying to make the racial zoning ordinances work to the benefit of the city's black communities.

Silver and Moeser's strength in spatial decision analysis is counterbalanced by their weakness in political analysis. At times, they appear naive in the political conclusions they draw. They seem genuinely surprised to find that, despite the size and unity of the black vote in the 1959 Memphis municipal elections, blacks were unable to elect a single candidate. Their conclusion for this outcome was that whites far outnumbered blacks in voter registration. I do not suggest that this conclusion is incorrect, because it clearly is not, but that it should be the beginning of the political analysis, not the end. Silver and Moeser are not able to provide the same nuanced portrait of the politics in these cities that they do of the decisions related to spatial population distribution. In addition, their assessment of the current political realities in the urban South is not as developed as it should be.

In contrast, James Button's chapter, "The Continuing Legacy of Discrimination in Southern Communities," in *The Bubbling Cauldron: Race, Ethnicity, and the Urban Crisis*, provides the depth of current political analyses that Silver and Moeser lack.[1] Button sees definite political progress for blacks in the South, but he also identifies the continuing barriers that inhibit this progress. Blacks, he argues, are losing ground in many areas in which they previously showed gains.

There has been a tremendous rise in the number of black elected officials, and this has caused a lessening in police brutality and an increases in municipal employment and services. Yet, many business owners and employees still treat blacks as second-class citizens. Furthermore, discrimination continues in the educational system, and segregated schools abound. In essence, little desegregation has taken place, particularly in the Old South. With less government intervention in private sector employment, Button suggests, prospects for improved black social and economic opportunities in the future are limited.[2]

EXTERNAL ECONOMIC FORCES: ECONOMIC INEQUALITY

Implicit in the analyses of Silver and Moeser and Button is the existence of discrimination in the economic arena and its resultant racial economic inequality. While most acknowledge the inequality, the reasons for it and the role of government in confronting it are points of contention. The books by Martin Carnoy, *Faded Dreams: The Politics and Economics of Race in America*, and Stephen Burman, *The Black Progress Question: Explaining the African American Predicament*, address aspects of black economic inequality with varying degrees of success. Carnoy's is a thoughtful and empirically based analysis of the factors responsible and the role of government in reducing inequities. Burman presents a theoretical treatise on what he sees as the central issue facing black Americans: the black progress question.

Carnoy argues that the three dominant explanations for inequality in the political environment—individual responsibility, pervasive racism, and economic restructuring—represent different ideological perspectives and take a different slant on the same data. Individual responsibility, associated with such conservative scholars as Thomas Sowell, Charles Murray, and Shelby Steele, starts from the premise that individuals have the power to control their life and destiny by their own choices. This explanation lays the blame for racial inequality on blacks who do not respond to economic opportunities and on white liberals who try to use government to make things better but only make them worse. The way to eliminate racial economic inequality is through the free market. Carnoy argues that this is an idealized construct, and the assumption that it works without regard to race or class is a fallacy (chapter 4). The individual responsibility approach conveniently overlooks the intimate tie between politics and markets, and that people's choices are limited by their own as well as market economic conditions.

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[1] This book is a collection of essays on various aspects of urban life. Not all are by political scientists, and not all are relevant to this review article. I highlight only two and footnote the results of a third. Yet, the entire volume is useful for political scientists, sociologists, anthropologists, and other scholars interested in urban life and politics.

[2] The chapter by Cedric Herring and Sharon Collins provides empirical evidence from Chicago of the positive effects of affirmative action on the economic elevation of previously excluded racial groups. Also, they found the absence of a zero-sum effect, that is, the elevation of the status of racial minorities does not detract from the chances of white males to succeed. They also conclude that the withdrawal of the federal government from the employment arena will decrease employment opportunities for racial minorities.

On the other end of the ideological spectrum is the pervasive racism explanation, associated with, among others, the work of Andrew Hacker. It argues that racism is part of the very structure of society, not just the pejorative attitude that one individual or group has toward another of a different race. Institutionalized racism stymies black progress and is so pervasive that even if individual racism were to decline, it would still exist. Carnoy levels the same criticism against this explanation as he uses against the individual responsibility argument (chapter 6). Each assumes an idealized social construction. In the case of pervasive racism, the ties between government and politics are not assumed away, but they are so institutionally rigid and inherently racist that change occurs only in the face of intense pressure from minority groups, and only temporarily.[3] The previous rigidity resumes when the pressure recedes or counterpressure comes from the dominant white community. Empirical historical evidence, Carnoy contends, goes against this view. Permanent changes have occurred in the way the political system deals with the issue of race. Government has been willing to push economic and social institutions to change their behavior during certain periods. He recognizes, however, that such willingness waxes and wanes with each presidential administration.

Economic restructuring, prominently associated with the work of William Julius Wilson and Robert Reich, suggests that racial inequality is the result of change in the business sector. Businesses now are using more sophisticated, high-tech equipment that requires more highly educated workers. A large segment of the black population is at an increasing disadvantage, along with nonblacks of similar education levels. The problem, then, is not one of racial inequality but of growing class inequalities due to economic restructuring. In this explanation, government plays a positive role in improving minorities' economic position by increasing access to higher education and creating employment opportunities through direct federal and state programs.

According to this view, changes in economic conditions have halted the successes of earlier programs, which are now obsolete, and new approaches need to be developed to reach those segments of the population—both black and white—excluded from employment opportunities. Since poverty is now based on class not race, race-based government action no longer works because everyone who can take advantage of such programs has already done so. Carnoy takes issue with this explanation as well (chapter 5). He argues that while many blacks have moved into the middle class and are better off in absolute terms than those they left behind, they are worse off compared to whites with whom they are now competing. Moreover, class politics in the United States has never been separate from race politics.

Ultimately, Carnoy argues for an explanation that takes into account the relationship between politics and markets. In essence, he integrates aspects of all three viewpoints. Politics, he argues, is key to explaining racial inequality. He concludes that economic inequality in the United States still has an important racial component and that a government with the will to reduce racial inequality can and should do so. Government has the capacity to invest in public education in ways that are favorable to low-income Americans and even more so to low-income minorities. It can affect access to credit for private investments, can support macroeconomic policy that lowers general unemployment, has the legal power to equalize opportunity in labor markets, and can set the standard on wages and employment. This can be done in ways that favor low-income and minority groups. For Carnoy, market forces are inseparable from political ones.

Carnoy is one economist who puts economic analysis within the context of the political system. While those ideologically tied to an explanation may find fault with his analysis, his work is balanced and empirically grounded. *Faded Dreams* has much to say to political science, and it contributes to our knowledge of the politics of economic inequality.

Whereas Carnoy contributes an economic perspective to the study of racial politics, Burman argues that U.S. race relations need to be examined in a global context.[4] Specifically, he grapples with the ways to define progress. One definition is the complete integration of blacks into U.S. society, which denotes color-blindness and the allocation of wealth and status solely according to individual merit; discriminatory characteristics such as race, ethnicity, gender, and sexual preferences no longer have currency. Another potential definition of black progress is separatism with equality, that is, equal poverty levels or socioeconomic profiles as between whites and blacks without integration.

These definitions, Burman suggests, are impractical and show the pointlessness of attempting to reduce black progress to a simple definition. This difficulty in defining black progress has resulted in different theoretical traditions that attempt to explain the position of blacks. The book proceeds to examine these—liberalism, neoconservatism, Marxism, Black Nationalism, radical culturalism, and pluralism—and their usefulness in addressing the question of black progress.

Burman's discussions of the evolution of the liberal and pluralist traditions are very well done, but Burman betrays his unfamiliarity with the black politics literature in his discussion of black politics and pluralism. His citations do not go beyond the late 1970s, and thus he misses the voluminous and arguably the most important work challenging the appropriateness of pluralism as a framework for the study of black politics, particularly the excellent book by Dianne Pinderhughes (1987). What is more important, it is obligatory for scholars writing on black politics to have at least passing acquaintance with the literature.

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[3] Carnoy's observations are comparable to those of Piven and Cloward 1977.

[4] Although he argues for a global context, Burman focuses almost exclusively on the predicament of U.S. blacks.

Burman's discussion of the limitations of the radical cultural approach to examining black progress are engaging but somewhat restricted in scope. In this area are Black Nationalism, as reflected in the work of Essien-Udom and Carmichael and Hamilton, and left liberalism, exemplified by Henry Louis Gates, Jr., and Cornel West. Burman sees Black Nationalism as having been transformed into a conservative vehicle for middle-class black advancement, while left liberalism uses the importance of black culture to a genuine multicultural America as a gateway to political inclusion. The brevity of his discussions, however, results in unidimensional descriptions of complex theoretical approaches. He concludes by arguing that no solution to the black progress question is in sight and that each tradition offers no relief for the plight of black Americans. Black progress has been ambiguous, possibly even reinforcing continuing subjugation. He sees the future of black America as confined to what he calls "semi-progress." This book, unfortunately, offers little to an understanding of the magnitude of the issues confronting black politics today.

EXTERNAL STRUCTURAL FORCES: FAILED STRATEGIES, NEW DIRECTIONS

The issues confronting black politics are not simply the inability to define black progress, as Burman would have us believe. The problems are complicated, and it is precisely this complexity that demands an evaluation of the years after the passage of the Voting Rights Act of 1965. To use an old cliché, "you cannot know where you are going until you know where you have been." Robert Smith's *We Have No Leaders: African Americans in the Post-Civil Rights Era*, and the chapter by Adolph Reed, Jr., "Demobilization in the New Black Political Regime: Ideological Capitulation and Radical Failure in the Postsegregation Era," in *The Bubbling Cauldron* are two such works. Yet, Smith and Reed come to very different conclusions about the effects and importance of the past three decades and the future of black politics.

Smith, anchoring his book in the social movements' literature, recounts the development of the modern phase of the civil rights movement from W. E. B. Du Bois and the Niagara Movement in 1905 up to 1965. His conclusion is that the response of the system to the demands for civil rights was slow and symbolic, prompted only when protests, violence, and disorders threatened or were perceived to threaten the stability of the system. The civil rights movement ended in the mid-1960s, and the goals established in 1905 and achieved through lobbying, litigation, and protest were virtually realized. Smith's focus for the rest of the book is on assessing what has happened since. His organizing question, stated very broadly, is whether incorporation of blacks into the political system and coalition formation with others have made a difference.

Using archival and interview data, Smith addresses the various venues and events in what he calls the institutionalization of the civil rights movement. From the National Black Political Conventions (1972–84) to the formation of the Congressional Black Caucus (1969) to the Joint Center for Political and Economic Studies (1970) to the NAACP to Clinton and the Democratic Party's abandonment of liberalism, Smith painstakingly leads the reader through the history and politics of the primary players in the post-1965 struggle for black inclusion in the political system. Black political leadership, Smith asserts, has been ineffective in maintaining viable and effective organizations, which began to tune their message to the concerns of an emerging black elite and middle class and to the foundations that provided funding. Rather than seriously address the problems of lower-income and poor blacks, the elites used those problems as leverage with the larger white political elite. Black political leadership has lost its compass, if it ever had one.

Reed also is critical of the effects of the activities of this era, but from a different viewpoint. He perceives a double-edged sword from the achievements of what he calls the "postsegregation era." While the number of black elected officials at all levels has grown substantially since the passage of the Voting Rights Act of 1965, he also sees a major drawback to political incorporation—the demobilization of black politics. In other words, benefits to particular sectors of the black electorate (i.e., black elected officials) have resulted in a dampening of the desire to confront the system. Black political incumbency constricts the field of political discourse.

In contrast, Smith declares that the events since 1965 have been irrelevant to the advancement of poor blacks. So as not to misrepresent his conclusions, I quote Smith: "This book provides dismal, detailed evidence of the irrelevancy of black politics in producing in the last twenty-five years benefits for most blacks, especially the imperative to reconstruct and integrate the ghettos into the mainstream of American society" (p. 273). Where did things go wrong, and why this total and complete failure? What route should black leadership have pursued instead? Smith argues that Black Nationalism or, as he says, the principles of self-help, self-reliance, and group solidarity should have been the approach rather than, I assume, the integrationist, political incorporation approach pursued.

Smith never makes clear which version of Black Nationalism he advocates. Is it that of Booker T. Washington, who argued for self-reliance and economic advancement but with the caveat that blacks not participate in politics? The nationalism of Louis Farrakhan, whose philosophy of self-reliance is tinged with racial animus? The nationalism of the Republic of New Africa, which seeks land for a separate black nation and reparations from the United States for the indignities of slavery, segregation, and other harmful acts? Perhaps it is the nationalism of Robert Woodson of the National Center for Neighborhood Enterprise or Clarence Thomas, whose movement to conservative Republicanism was merely the removal of a racial orientation from their nationalist ideology? Which of these variants would have produced more favorable outcomes for black Americans and the black lower class in particular?[5]

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[5] On another point, I take issue with Smith's uncritical use of "black underclass" as the shorthand term to describe poor and low-income urban blacks. Although not directed at Smith, Reed also criticizes the

Another criticism of Smith is his failure to address the latent, and often active, sexism of some aspects of Black Nationalist ideology. I am particularly skeptical of a tradition being extolled as the definitive strategy for black advancement that has historically viewed black men as primary and black women as minor players in the struggles for equality. Black Nationalism and many of its proponents fail, or often refuse, to acknowledge that gender issues must be confronted by black Americans. The view that gender questions are "created" by bored middle-class white women and are not shared by black women remains far too prevalent within the nationalist ranks. (Many, sadly, will recall Stokely Carmichael's response, several decades ago, to the question of the role of women in the movement.) Smith does not address this grave failing of nationalist ideology. My assumption, therefore, is that the gains he assumes would have accrued from pursuing this approach would have come at the expense of the humanity, political aspirations, and needs of black women. The failure to acknowledge and take into account issues of gender, and the interaction of race and gender in black scholarly and practical politics, is a deficiency in the black politics literature overall, and Smith does nothing to remedy this situation.

Despite these criticisms, Smith has written a very important book, one with which scholars of both black and American politics generally must engage. Smith is provocative, and scholars and reviewers will be provocative in turn. He poses the question of whether separation, in some fashion, from the larger body politic may produce more tangible rewards for the black poor than have previous strategies aimed at integrating elite and middle-class blacks into the system. This is a book that will generate intense controversy within the field and will force black politics scholars to talk about alternative strategies for political progress as well as the ideological and public policy consequences of those strategies. I compliment Smith for troubling the waters and generating debate that will be beneficial to the study of black politics, for it is only through argument that the field remains vibrant and dynamic.

On the flip side, Reed castigates the "self-help" ideology that Smith advances. From Reed's perspective, that ideology, interconnected with the rhetoric of the underclass, is the primary reason for the demobilization of black communities around the country. He argues that it perpetuates the mythical notion of a monolithic "black community," which preempts questions of interest differentiation among blacks. Whereas Smith sees Black Nationalism (black power) as the only course to pursue, Reed sees it as rhetoric that seeks to elevate the mythical notion of a singular black community as a source of political legitimation. Reed also sees such an ideology as advocating a withdrawal by blacks from a political system in which they are fully entitled to participate.[6] On another front, Reed criticizes the white left for its approach to black politics, namely, failing to recognize interest and activity differentiation among blacks.

CROSSROADS AND CROSS-HAIRS

The books reviewed here, although not focused specifically on current issues of black politics, nonetheless contribute to our understanding of current problems. They highlight the continuing effect of external political and economic forces on the black political condition and the growing influence of internal political dynamics on changing that condition. King provides the continuity of past problems of bias in the federal Civil Service with similar problems in the federal service today. Carnoy and Burman highlight the politics of economics and its effects on continuing black economic inequality. Silver and Moeser and Button identify the continuing presence of discrimination and separation in southern cities. Smith and Reed reflect on the events since the end of the civil rights movement and on the successes and failures for the politics of black Americans. Taken together, these studies recognize that history affects the future and that historical effects linger and often perpetuate themselves.

Currently, black politics, both as practical politics and as scholarly endeavor, finds itself at the crossroads, a crossroads with a puzzling set of dilemmas. Few would argue that blacks have not made a certain degree of political progress in the past three decades. Yet, many are questioning some of the strategies which produced that success as unfair to whites or outdated. What new strategies should be pursued in an environment unsympathetic to the policy concerns of black America? Are the goals of racial integration and political incorporation passé, and is separatism, as Smith argues, the path blacks should follow? Recently, the school board of Prince Georges County, Maryland, narrowly passed a measure abandoning racial balance in schools and releasing slots in magnet schools reserved for whites to be filled by blacks on a waiting list for admission. Is "separate but equal" on the rebound?

Simultaneously, black politics finds itself in the cross-hairs of the debate over whether race is still a salient factor in America. As such, should race be a consideration in decisions in the political process? The recent spate of Supreme Court decisions on admissions criteria for law school (*Hopwood* v. *Texas*) and majority-minority electoral districts in Texas (*Bush* v. *Vera*) and North Carolina (*Shaw* v. *Hunt*) answered the question with an inconclusive "no." These decisions, and the resultant heightened debate, raise a broader question for black and American politics and theories of representative democracy: What does representation mean, and what does it mean to be represented? These two questions, I predict, will dominate research in black politics over the next several years.

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rhetoric of the underclass and its use in political discourse. Herbert J. Gans (1990), once an advocate of the term, argues that the definition has become so hopelessly polluted in meaning and politicized that scholars should cease using it. He states, contrary to Gunnar Myrdal's (1962) use of the term as a purely economic concept (chronically unemployed, underemployed, and underemployables), it now has taken on many connotations of undeservingness and blameworthiness.

[6] Reed makes many of the same arguments used by Burman regarding the weaknesses of Black Nationalist ideology, but in a more systematic and eloquent manner.

The challenges facing black politics are monumental and, some would argue, insurmountable. Past opponents of racial equality and equal rights now cloak themselves in the tenets of the Civil Rights Act of 1964 and claim Martin Luther King as their patron saint. People should be judged by "the content of their character," not the "color of their skin." Policies that recognize people as members of a group, they argue, often disingenuously, are contrary to the teachings of Dr. King and the goals of the civil rights movement. U.S. society should be color blind, and we should begin by repealing every statute and policy that appears to favor blacks and other persons of color (e.g., the California Civil Rights Initiative). The external structural barriers to black social and political progress of the past no longer exist. These are the ideas that have currency in the present political environment. Still, the rhetoric of a color-blind society is, in many instances, a euphemism for the process of removing protections and rolling back gains made by black Americans.

The idea that the responsibility for removing group identity status lies with blacks themselves, not with the larger dominant society, is an explanation that belittles or completely ignores the cumulative and pernicious effects of racism in the U.S. social fabric. White residents of the south Philadelphia neighborhood who recently threatened and harassed a black woman and her children to the point that they eventually moved did so because she was black, a member of a group they despised. Who she was as an individual or whether she personally believed in a group identity or even thought in those terms was not the issue. The issue was that she was black.

The poignant autobiography of Gregory Howard Williams (1995), Dean of the Law School, Ohio State University, brings front and center the extent to which race is a social and political construct that defines blacks as members of a group. Williams brings the reader face to face with the reality of how the identification of him as black, despite his outward appearance of being white, changed his life chances and how society treats him. When people believed Williams was white, they deemed him bright, smart, and a future star. When they discovered he was black, the adjectives no longer fit, and doors that once were open, closed. He found himself judged and treated as a member of a group, a group that was to be discriminated against and limited in opportunities. As Williams so eloquently and painfully proves, the penalties for black group membership in the United States far outweigh any perceived or real benefits.

A larger question, however, about the current language of a color-blind society is what to do about the real differences that continue to exist. What do we do about the racial differences in infant mortality rates? What do we do about differences in high school curricula that provide many white high school students with the college preparatory base that many black high schoolers are unable to get? What do we do about minority representation in the presence of racially polarized voting? The list is endless. Unfortunately, the current debate about a color-blind society assumes that any racial differences that exist are the result of individual initiative or lack thereof, and that external structural barriers have been eliminated. If this were true, the locus of the problem would be within U.S. black communities, thereby eliminating the necessity for public policy responses. Nevertheless, evidence exists that this is a myopic assumption. Surely internal problems exist, but even if they were all resolved external barriers to black advancement would still remain.

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Social Movements in Contentious Politics: A Review Article

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Political Protest and Social Change: Analyzing Politics. By Charles F. Andrain and David E. Apter. New York: New York University Press, 1996. 387p. $50.00 cloth, $19.95 paper.

The Politics of Social Protest: Comparative Perspectives on States and Social Movements. By J. Craig Jenkins and Bert Klandermans, eds. Minneapolis: University of Minnesota Press, 1995. 381p. $49.95 cloth, $19.95 paper.

New Social Movements in Western Europe: A Comparative Analysis. By Hanspeter Kriesi, Ruud Koopmans, Jan Willem Duyvendak, and Marco G. Giugni. Minneapolis: University of Minnesota Press, 1995. 310p. $54.95 cloth, $21.95 paper.

Shanghai on Strike: The Politics of Chinese Labor. By Elizabeth J. Perry. Stanford, CA: Stanford University Press, 1993. 327p. $42.50 cloth, $16.95 paper.

Popular Contention in Great Britain, 1758–1834. By Charles Tilly. Cambridge, MA, and London: Harvard University Press, 1995. 476p. $49.95.

Ikki: Social Conflict and Political Protest in Early Modern Japan. By James W. White. Ithaca, NY, and London: Cornell University Press. 1995. 348p. $39.95.

Amid the many "turns" in the social sciences over the past few decades—most of them only briefly taken—there has been an extraordinary *re*-turn: the rebirth of systematic comparative and historical studies of contentious politics. I use the word *return* not because studies of social movements have been absent but because, since their heyday in the 1960s, most of these have been case studies of individual movements within a relatively short historical compass. In addition, many have left the complex and multifaceted relations between movements and political structures underspecified and badly operationalized. These new books, in contrast, all attempt to place social movements within a broader structure of contentious politics.

"Contentious politics" I define as collective activity on the part of claimants—or those who claim to represent them—relying at least in part on noninstitutional forms of interaction with elites, opponents, or the state.[1] "Social movements" I define, with Tilly, more narrowly as sustained challenges to powerholders in the name of a disadvantaged population living under the jurisdiction or influence of those powerholders (p. 369). Relating social movements to all forms of contentious politics should enable us to locate the former more effectively in relation to institutions, political alignments, and long-term political struggles; this is why, along with most of these authors, I call for a "political process" model of social movements. But as I shall argue later, such a move demands that we be more precise about the relations between movements and institutional politics—a conceptual task that remains to be completed.

With their pedigree stretching back to the work of Eisinger (1973), Lipsky (1970), Piven and Cloward (1977), Tilly's earlier work (1978), and sociologists such as McAdam (1982, 1988) and Gamson (1990), these books and several other recent ones link the study of social movements explicitly to politics.[2] Central to all of these studies is the concept of "political opportunity structure."[3] This is a concept that still needs better specification and operationalization; but it is the theoretical pivot that has allowed recent students of social movements to connect their subject to institutional politics—a far cry from the older "collective behavior" tradition—and distinct from the more recent traditions of "resource mobilization" and "new" social movements.

The revival of social movement studies that we have been witnessing is the more striking because it does not depend, as was the case in the 1960s, on the outbreak of a major cycle of protest.[4] If this has rendered this group of studies less inspirational than those of the 1960s, it has allowed them to be more systematic, more historical, and more comparative. In these six books, we can trace the halting, tentative, and sometimes problematic emergence of a political process model of social movements. It is a move that promises to bring the study of movements back to its original heartland—the political struggle—but it also poses some serious problems of conceptualization and comparison to which I will return below.[5]

The new approaches are characterized by three major elements. First, they often employ more refined methodologies designed specifically to analyze contentious interaction and take advantage of the advances in cheap and rapid computation over the past decade, what I will

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[1] For the justification for so broad a definition and for a statement of the ambition to compare social movements with collective action and revolutions within the same framework, see Tarrow 1994 and McAdam, Tarrow, and Tilly 1996a and b.

[2] See the recent books by della Porta (1995), Rucht (1994), and Tarrow (1994) and the collective volumes edited by Dalton and Kuechler (1990), McAdam, McCarthy, and Zald (1996), and Traugott (1995).

[3] The concept is more explicitly developed and employed in the books reviewed here by Andrain and Apter, Kriesi and others, Tilly, and Jenkins and Klandermans, and less explicitly in Perry's and White's books. For the origins and development of the concept of political opportunities, see Eisinger (1973), McAdam, McCarthy, and Zald (1996), and Tarrow (1994). For applications to particular movements and areas of the world, see Brockett (1995), Costain (1992), Katzenstein and Mueller (1987), Kitschelt (1986), and Tarrow (1989).

[4] Some readers of a draft of this essay have objected that events in the Philippines (1986), Burma (1988), China, Eastern Europe (1989), and elsewhere constitute a cycle of protest. Be this as it may, most of these studies were begun, or at least conceived, before the latest wave of movements broke out or came from totally different areas of the world.

[5] The term "political process model" was first suggested by McAdam (1982) in relation to the U.S. civil rights movement. Its relationship to the key concept of opportunity structure, and to structural approaches to movements in general, is discussed in McAdam, Tarrow, and Tilly (1996b) and in Tarrow (1988).

call contentious event analysis.[6] Instead of pursuing a group and telling its story from the perspective of the movement or the observer, event history practitioners like Tilly, Kriesi and others, and White compile databases from published accounts in the contemporary press on those events that they consider worthy of notice. This provides them with a temporal map of incidents through which the movement's activities and interactions can be traced and through which movements can be related to relevant covariates and political contexts.

Second, after years of psychological, social-psychological, and resource mobilization approaches, these books bring sociologists and political scientists together in collaborative and replicative work. This is a departure for movement specialists from both disciplines, two groups who had been growing apart in recent years—political scientists in the direction of economics and sociologists toward organization theory.

Third, instead of the familiar case study approach that dominated the field in the past, these scholars all bring a broadly comparative and historical framework to the study of social movements.[7] Let us begin with this most notable feature of these books, their embedding in comparative and historical frameworks of interpretation.

COMPARATIVE AND HISTORICAL PERSPECTIVES

If these six books turn out to be representative, then the tradition of the movement-career case study has been seriously challenged. In the case study tradition, scholars who are, or once were, associated with or strongly opposed to particular movements typically fasten on a movement or movement organization, trace its career from spontaneous beginnings to institutionalized ends, and identify the agents responsible for this dynamic.[8] Three of the books under review (by Andrain and Apter, Jenkins and Klandermans, and Kriesi and others) are explicitly comparative; of the three that focus on one country alone, all are at least implicitly comparative and cover periods of time that go beyond the life of a single movement organization. Tilly's book on Britain is packed with comparative material and is part of his long-term project on claims-making in Britain and France[9]; Perry's on China follows a heterogeneous social actor across a century of changing economic, social, and political systems and compares it in three very different industries—textiles, ports, and tobacco; and White's on Japan is structured as a moving multivariate relationship between peasants and the structural trends in their society over three centuries.

The importance of this comparative dimension for the study of contentious politics cannot be overstated. Social movements are historically evanescent and, as a result, scholarly interest in them waxes and wanes almost as rapidly as they do. This has produced a wealth of idiosyncratic case studies that cannot be compared systematically to those in other countries or periods of history and are therefore doomed to appeal to narrow audiences and to have short shelf lives. By focusing on movements comparatively, the books under review promise their authors longer futures and broader readerships.

With respect to history, there is something new too: several of the books employ rigorous statistical methods within historical perspectives. In the past, historical studies of social movements centered on the history of the particular movement, with general political history as the backdrop. By looking systematically at contentious events over time, authors such as Kriesi and his collaborators, Perry, Tilly, and White focus on the intersections between contending actors and their opponents in different political contexts. From the history of particular movement organizations with politics in the background, all six books shift our attention to the histories of the contentious interaction between movements and the polity. Let us summarize them briefly to examine how their authors do this.

TILLY'S TWO RHYTHMS: ENUMERATING TALES OF CONTENTION

Both in its comparative framework and in its historical framing, Charles Tilly's *Popular Contention in Great Britain* is exemplary. Focusing on more than 8,000 contentious gatherings of ten or more people observed from contemporary sources in the greater London region during thirteen sampled years from 1758 to 1820, and in Britain as a whole from 1828 to 1834, Tilly has elaborated a model of contentious event analysis over many years of experimentation and analysis (p. 63). The method, later adopted in broad outline by Kriesi and others, White, and others,[10] is to enumerate and analyze the actors, their actions, and the targets of their contention from serial sources in the context of the political struggles of their times and to relate the data to covariates drawn both from the same sources and from other serial and contextual data. Tilly, in whose work sociology always meets history, leans more extensively on contextual than on serial data; but in the hands of a scholar like White or Olzak (1992), the method allows sequences of contentious events to be related to structural time-series trends like food prices, immigration, urbanization, and the growth of capitalism.

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[6] Rather than borrow from electoral, organizational, and other fields of study, three of the studies under review employ this methodology. For reviews of the instruments and the kinds of data typically used in event history analyses, see Olzak 1989 and Tarrow 1996.

[7] *Inter alia*, this has brought students of U.S. movements squarely into contact with the work of European scholars, helping to lead them out of an exceptionalist mode and producing major indirect as well as direct benefits of comparison. Costain (1992) and McAdam (1982) are two of the scholars whose American-based work is conceptually linked to comparative research on European movements. Direct transatlantic collaboration can be traced to the 1980s and work of scholars brought together by Dalton and Kuechler (1990), Katzenstein and Mueller (1987), and Klandermans and others (1988). Direct comparisons of U.S. and West European movements are those of Kitschelt (1986), McAdam and Rucht (1993), and Rucht (1994).

[8] In the best work in this tradition, for example, Piven and Cloward's *Poor People's Movements* (1977), a model of institutionalization was applied comparatively to four movements in recent U.S. history. But in less skillful hands, the approach can degenerate into telling stories of the rise and fall of a movement organization, sometimes blaming the agents responsible for its institutionalization and sometimes celebrating its achievements.

[9] See Tilly (1964, 1986) and Shorter and Tilly (1974) for his major works on French contention. Tilly has also provided the most historically rooted version of contentious event analysis and has directly influenced most of the other works under review.

[10] Similar methods have been used by Olzak (1992), by her students Olivier (1989) and Soule (1996), and by Tarrow (1989), as well as in important work in progress by Beissinger (1993, 1996), Ekiert and Kubik (1995), and Rucht (1996). The latter two studies directly replicate important parts of the research protocol developed in Tarrow (1989) for Italy.

Tilly's focus is more directly on the contentious data themselves. He shows that the secular trend of contention as Britain moved into the nineteenth century lay in a shift from the local, parochial, bifurcated, and particular—and often violent—gatherings of the early part of the period to the cosmopolitan, autonomous, modular—and usually peaceful—repertoire of its close (pp. 45–8). He focuses not only on the "long rhythm" of changes in Britain's contention but also on the shorter rhythms of a number of crucial cycles of protest, including the one that gave rise to Britain's modern politics, 1828–34 (chapter 7). This period emerges from Tilly's method not, as many have thought, as a single peak of contention centered on the Reform Act of 1832 but as a partly autonomous and partly linked series of hills and valleys, including the struggles over Catholic emancipation and Protestant reaction, the suffrage reform, and the Swing movement. In the process, argues Tilly, the social movement organization, soon to become the staple of modern contentious politics, was born.

But although Tilly makes a reasonable case that the changes in contention correlated roughly with the growing centralization of the national state and the capitalization of the British economy, the reader will look in vain for a statistical test of the association between changes in capitalism and the changes in the nature of contentious gatherings. Capitalism only hovers in the background of Tilly's account, in part because he has not included strikes in his enumerations, but in part through an explicit choice: "For thirty years," he notes, "capitalism has dominated the discussion, and I want to redress the balance."[11]

As for changes in the state, these operate in two directions, internal parliamentarization and external warmaking, both of which show up dramatically in the changes Tilly finds in the mounting of collective action. This, on the one hand, is increasingly directed at Parliament and, on the other, is greatly affected by war and by the strains of war financing. But despite the wealth of data he collected, these correlations are only illustrated and are never demonstrated statistically.[12] A major reason for this is the small number of sample years studied (thirteen sampled years for the London region from 1758 to 1834 and seven consecutive years for the shorter national sample between 1828 and 1834). But another is that, once having established the long rhythm of changes in the repertoire, Tilly's attention is captured by shorter rhythms, cycles of protest, and particular epochs of political history, like the reform period. The weakness of the book is in its failure to relate the "long rhythm" of his database systematically to relevant covariates; its strengths are in tracing the fundamental change in the repertoire as Britain modernized and in carving out a strategy for the contextualized study of events-in-history that can be replicated and compared to other countries, like China and Japan.

ASIAN ACTORS: PERRY AND WHITE

More locally based and written in a more narrative mode than Tilly's book, but equally embedded in particular structures of contention, is Elizabeth Perry's inspiring book, *Shanghai on Strike*. Though influenced by the work of such cultural historians as Hunt (1984), Perry actually has more to say about the effects of a structural factor emphasized by sociologists such as Gould (1995) and McAdam (1988): social networks. But rather than focus only on conventional movement organizations, Perry shows the key role that guilds, criminal gangs, and native-place associations played in the development of the Shanghai labor movement. And alone among these authors, she shows how gender intersected with skill hierarchies and native-place identities in this development. Although the theory in her book sometimes needs to be teased out of the narrative, Perry shows how these "natural" proclivities took on a changing political significance depending on the political context, most notably in the partisanship of first the KMT and then the CCP.

As in Tilly's book, events are the major data points in Perry's account—strikes in her case. But although she looks at contention for 110 years, her book has no equivalent for Tilly's "long rhythm," for she contents herself with more or less discrete period analyses, as in Tilly's "short rhythms." These studies of particular periods in Shanghai's labor history are richly detailed and deeply researched. If the book has an overall theme, it is the uneasy tension in the Shanghai labor movement between solidarity and fragmentation. Labor historians have often taken polar positions on the issue of class solidarity, from the impossible-ism of mainstream U.S. historians (e.g., the position that the fragmentation of the working class, which was due to ethnic and racial differences, was a fundamental bar to class solidarity) to the inevitable-ism of the Marxists (e.g., the conviction that the concentration of capital would produce an inevitable growth of solidarity). Perry engages these schools, but she explores the ways in which a fragmented class can act in a politically solidary fashion under various structures of political opportunity. But she does not see workers engaged in a linear developmental process in which they gain "consciousness" and revolutionary potential.[13]

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[11] In a personal communication to the author, commenting on an earlier draft of this paper.

[12] Tilly writes that he decided to forswear the modeling and estimation of causal relationships in this book to disencumber the narrative, speak to historians of Britain, and give himself the discipline of laying out in words "what quantitative modeling will eventually have to represent, verify and falsify" (p. 73).

[13] Perry's concluding discussion is worth quoting in detail: "The very awareness of substantial differences among workers often encourages labor activism. . . . Even in the important instances when workers at different skill and wage levels cooperate in joint struggles, the alliances

But does Shanghai's labor history reveal no more than a series of contingent coalitions among otherwise fragmented groups? Or did the act of participating in Perry's short rhythms create increasingly broader collective identities and action frames? What of the long rhythm of the city's industrial tradition? Did it induce no permanent political culture of rebellion? It is striking that a tradition of strikes survived through every phase of Shanghai's history, including some fairly repressive ones. Perry's narrative method penetrates discrete historical periods defined by their dominant political conflicts, but, like Tilly's "short rhythms," it leaves us in the dark about the cumulative growth of a tradition of rebellion.

Even more impressive for its historical and geographic breadth than Perry's book, and more statistically structured over the *longue durée*, is James White's analysis of Japanese rural revolts between 1590 and 1877. Declaring his debt to Tilly (p. xi), White bases his analysis of contentious events on a well-known Japanese archive, Aoki Koji's historical data set on rural rebellion, which in turn was compiled from a wide variety of local and national sources on legal, nonviolent, disorderly, and aggressive contention for almost three centuries (count them!). White roots his study in the tradition of early modern Japanese economic history, but more explicitly than Perry, he connects it to current social movement theories.

White's book shows how quantitative social science methods can be creatively married to in-depth historical knowledge of a particular country. Unlike either Perry or Tilly, he engages in the use of complex multivariate models using data aggregated at both the county and the provincial level. This provides White with a common metric for a long historical period, allowing him to compare the effects of rice shortages, inflation, unemployment, and low wages upon peasant unrest and rebellion. But it will have costs for this otherwise exceptional book. First, it will probably lose White the readership of historians who are more comfortable with narratives of discrete periods than with the statistical study of the *longue durée*. Second, in the absence of indicators of individual behavior, White improvises with a number of purpose-built ones drawn from census data whose relationship to individual-level variables (or its absence) can only be surmised. Third, there is no overarching theme to carry the reader along from one short rhythm to the next over a vast historical terrain.

Still, this path-breaking study marries a superb data set with the skills of a historically trained social scientist. With the exception of one now-dated and rather simple analysis of collective action events after World War II (Sugimoto 1981), it breaks new ground in the statistical analysis of contention in modern Japan. And its openness to rational choice perspectives (see below) will provide a benchmark for years to come for statistically trained Japanese social scientists to test the sometimes facile generalizations about their country that come from the now dominant culturalist tradition in Japanese historiography.

NEW EUROPEAN MOVEMENTS

There was a period in the 1980s when European scholars like Offe (1985) and Melucci (1989) saw a new wave of social movements developing out of the changes in advanced capitalism—movements that had been liberated from class and ideology; that employed new and creative forms of action; and that focused on identity concerns rather than on strategy (Cohen 1985). The resulting "new" social movement paradigm had good fortune among scholars in Western Europe and even in parts of the world where its social structural preconditions (e.g., advanced liberal capitalist states) were absent. So goes it with new trends in research; but the following decade saw these movements either weaken considerably or take a "long route through the institutions" without, however, inducing their theorists to revise their sometimes apocalyptic vision to relate the new movements more systematically to routine politics.[14]

Now, for almost the first time, in the book by Hanspeter Kriesi and his collaborators,[15] we have an empirical analysis of Europe's new social movements which relates them directly to institutional politics. More modest historically than the three studies previously discussed (the book covers a sample of protest events for fifteen years from the mid-1970s to the late 1980s) *New Social Movements in Western Europe* is the only study in this group based on a systematic comparative design. The authors have collected parallel data on protest events from newspaper files for France, Holland, Switzerland, and West Germany from 1975 to 1989 and analyzed them in relation to a battery of indicators of conventional politics. Using a single newspaper source and a sample of dates for each of their four countries, they work with a rather simple research protocol but relate their findings creatively to the characteristics of the political systems of their four states.

Not the least of this book's virtues is that its authors specify the much overused label "new social movements" more clearly than their European predecessors tended to do. They demonstrate the junctions between these movements and the not-so-new institutional politics of the countries they study. Far from being detached from the political process as the more ardent students of these new movements imagined, Kriesi and colleagues show how the rhythms of the contentious events that were mounted over a decade and a half corresponded closely to the different party alignments in each country—from France's Socialist government's successful coopting of its new social movements in the early 1980s to the West German government's provision of opportuni-

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do not necessarily reflect class consciousness. . . . The mobilization that led to massive struggles was usually based on pre-existing, smaller-scale groupings. Only when fictive kinship networks, native-place gangs, secret societies, and the like could be drawn into cooperation was a major upheaval possible" (p. 251).

[14] See the partial revision of his theory in the guise of an updating by Offe (1990) and the critiques of the theory's ahistoricism by Calhoun (1995) and D'Anieri, Ernst, and Kier (1990).

[15] They are Jan Willem Dyvendak, Marco G. Giugni, and Ruud Koopmans.

ties to similar movements through its support for the European missile system. This book is a model for the comparative study of social movements in relation to the processes of conventional politics.

At the heart of the study is the concept of political opportunity structure, which the authors employ in both a cross-national and a dynamic cross-time manner to relate their findings about protest to more conventional institutional and social structural indicators. Unfortunately, they hew more closely to the static, cross-national operationalization of the concept than to its time-series version, which makes it easier for them to compare national political systems' influence on movements than to analyze changes in movement strategy and structure. They show clearly how differences in the character of states and cleavage structures have an effect on movements in the four countries—the basic message being that France is different. Except for a chapter on changing alliances (chapter 3) and another on cycles of protest (chapter 5), however, the static, cross-sectional specification of opportunity structure prevents them from taking analytical advantages of how changes in political alignments stimulated movement rise and fall over time.[16]

Be that as it may, Kriesi and colleagues demonstrate how movements respond to, and occasionally create, political opportunities in relation to standard variables of political analysis. This not only provides a healthy antidote to the obsessive "new-ism" of the studies of new social movements that appeared in the 1980s but also demonstrates the powerful uses of the concept of political opportunities for comparative politics; and it will eventually help us understand whether the last few decades of Western history witnessed a progressive increase in contentious politics, as was recently argued by Dalton (1996, chapter 4), or rather a more familiar pattern of protest cycles.[17]

TWO SYNTHESES

Standing apart from these four books both in style and content are the volume edited by Jenkins and Klandermans, *The Politics of Social Protest*, and Andrain and Apter's *Political Protest and Social Change*. The former delivers both less than it claims—to "move beyond existing social movement theories by examining the role of states in social movement development" (p. 7)—and more. While the editors' specification of the theoretical relations between states and social movements is too cursory and is not followed through consistently in the empirical chapters, Jenkins and Klandermans provide the reader with a broad sample what many of the new group of social movement researchers are writing, and they make a distinction that is often lost in this tradition—between the largely structural approaches used in most comparative studies and the processual focus in time-series studies of individual countries. As indicated above with respect to Kriesi and others, the attempt to analyze the processes of change from a static structural standpoint is one of the ongoing problems of the comparative political process approach.

The Jenkins and Klandermans book is divided into sections on the political origins of social protest, the electoral contexts in which protest occurs, and (a few of) the outcomes that movements sometimes achieve. Particularly notable are the three comparative chapters (the contribution of Karl-Dieter Opp and his collaborators on Germany, Israel, and Peru; that of Michael Nollert on neocorporatism and protest in Western democracies; and the essay by Donatella della Porta and Dieter Rucht on the "left-libertarian" movements of Italy and Germany from the 1960s through the 1980s).

Some of the chapters in the book synthesize work reported at greater length elsewhere: Kriesi provides a succinct chapter summarizing the major findings of his co-authored volume; Ron Aminzade offers a deft analysis of the relations between movements and parties in mid-nineteenth-century France (see Aminzade 1993); and Russell Dalton summarizes the findings on strategies of partisan influence reported in his *The Green Rainbow* (1994). Diarmuid Maguire draws on his study of the 1980s European peace movements (1990) to employ the political opportunity structure concept to great advantage. Jenkins provides a crisp theoretical chapter on movements, representation, and the state that goes beyond the "here is what our authors will say" format (chapter 2). But Klandermans, who has done more to create the current synthesis of West European and U.S. research than anyone else, contents himself with co-authoring a brief introduction with Jenkins.

Political Protest and Social Change also delivers both more and less than its authors promise. Like Kriesi and others and Tilly, Andrain and Apter "formulate a theory of political opportunities," one which assumes "that cultural values, socio-political structures, and individual behaviors shape the origins, activities, and outcomes of protests" (p. 3)—a tall order. But in contrast to Tilly, the book makes no attempt to apply the concept historically; and in contrast to Kriesi and others, opportunity structure is never well specified theoretically or even applied to concrete social movements. (In fact, between its first appearance in the Introduction and its apotheosis in the Epilogue, the language of opportunity structure appears only eleven times in 317 pages of text.) Given the centrality of the concept in the current social movement literature and to the authors' own theoretical position, one might have expected to learn much more about how opportunities shape movement mobilization, collective action, or outcomes.

The greatest strength of the Andrain and Apter book is the authors' incredible range of acquaintance with theory and research in comparative politics. They work out of three building-block concepts (structure, culture, and behavior). These they surround with syntheses from a plethora of theoretical approaches and issues, illustrating them with samplings from their own and others' empirical work from Africa, Asia, and Western Europe.

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[16] It also leads them to underplay the growing *transnational* influences on these movements that began after the 1960s, since their independent variables are almost all specified at the level of the national state. For evidence of transnational diffusion of the movements emanating from the 1960s, see McAdam and Rucht (1993).

[17] For alternative "cyclical" approaches, see Brand (1990), Koopmans (1993), and Tarrow (1989, 1994).

There are some choice morsels here, such as the synthetic chapter on ideologies (chapter 2), which has echoes of Apter's classical piece from his edited volume, *Ideology and Discontent* (1960), and the one on nationalism (chapter 4), which recalls his earlier work on political religion (1963). But there are also long, slogging essays that seem better suited to preparing graduate students for their general exams than for motivating and shaping empirical work on contentious politics. The notes and references alone would take most readers several years to cover.

In summary, these six books provide a rapid overview of the current state of theory and research on social movements in both Western Europe and the United States. Three of them (Tilly, White, and Kriesi and others) operationalize a familiar subject—social movements—through a systematic methodology—contentious event history. Two of them (Tilly and Perry) are deeply historical in a narrative sense. And three (Kriesi and others, Jenkins and Klandermans, and Andrain and Apter) are explicitly comparative. Together, they bring both history and comparison systematically into the study of collective action. But there are two major disappointments in these studies: their uncertain relationship to the rational choice paradigm that has been gaining grounding in our discipline, and the connections among social movements, contentious politics, and political institutions.

CONTENTIOUS POLITICS AND RATIONAL CHOICE: PARALLEL PROGRAMS OR COMPETING PARADIGMS?

In a definitive review article, Mark Lichbach writes that "in spite of numerous efforts by economists, political scientists and sociologists, most scholars now recognize that the marriage between the CA (collective action) research program and conflict studies has largely failed" (1994, 9). Why has this synthesis not occurred? One common element in all six studies under review is that they are far more contextualized, and less deductively streamlined, than most of the work that has come out of the choice-theoretical tradition. In these and other studies in the political process tradition, the guiding assumption is that movements arise, change, succeed, or fail as a function of changes in political opportunities. However operationalized (and this is a source of contention), a focus on opportunity structure produces a rich analysis of how political rules and institutions, strategic choices, and changes in the forms of contention affect social movements.

In the form in which rational choice theorists have approached contentious politics, which I will refer to with Lichbach as the "collective action" approach, this focus on the variable and changing interaction between movements and their interlocutors is radically foreshortened and comparatively impoverished. Apart from one or two works on peasants (Berejikian 1992, Popkin 1977), a reader on revolutions (Taylor 1988), a modest literature on strikes (Golden 1996), Chong's study of civil rights (1991), Opp's work based on survey materials (1989) and his study of the East German revolution (Opp, Voss, and Gern 1995), and Tong's innovative reconstruction of Chinese rebellions (1991), most researchers on contention who access the rational choice tradition have limited themselves to enunciating general laws (DiNardo 1985). The reason seems to be that the version of rational choice that has been imported into the study of social movements has "focused almost exclusively on the initial problem of whether anyone who is rational will actually participate in protest and rebellion" (Lichbach 1994, 9). The result, concludes Lichbach, "is that almost no CA theorists have gone on to study the many substantive problems arising in revolts and protests" (p. 9)—the very problems that are central to the works reviewed here.

In fact, for most writers coming from the rational choice tradition, collective action remains a generic term. This has the virtue of allowing its users to subsume a variety of kinds of action under a general theoretical rubric, but its subcategories are seldom distinguished from one another or related to the differential tendencies of different actors to employ them (e.g., Tilly's findings about the historical changes in the structure of the repertoire would be unimaginable for a theorist who posited something called "collective action" and called it a day). For most writers in the collective action tradition, individuals calculate the costs, risks, and constraints on collective action based on the nature of the goods they want to maximize and of their incentives and constraints to seek them; but they seldom take into account the incentives and constraints offered by particular opportunities or traditions of collective action, like the strike, the *charivari*, or the protest demonstration.[18]

The dominant paradigm of collective action theory is and remains the market.[19] This, in turn, leads to an inevitable caricaturization of CA on the part of scholars who find market models unpalatable,[20] a reaction that has not been helped by the tendency of some rational choice theorists to claim totality for their models and dismiss work coming from less deductively satisfying traditions as "atheoretical" (Kiser and Hechter 1991). Few scholars on either side of the divide have taken Lichbach's advice of trying to build bridges between the two research traditions (1994).

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[18] Some recent efforts have relaxed the microeconomic assumptions of the original CA model, focusing, for example, on the differential tendency to participate of people who are facing losses versus gains (Berejikian 1992); on the influence on willingness to participate of the likelihood that a campaign will succeed (Klandermans 1984); on the incentives to collective action that arise within communities (Taylor 1988); and differentiating the incentives to collective action in markets, communities, hierarchies, and contracts (Lichbach 1994, 1995).

[19] Lichbach makes an exhaustive effort to specify hypotheses in the collective action tradition based on his typology of hierarchies, communities, and contracts, as well as markets. The dominance of market metaphors in this area is evident, however, in the much larger number of hypotheses he summarizes under markets (N = 12) than under any of the other three types (communities = 2; hierarchies = 5; contract = 3). See his 1994 article, pp. 11–9.

[20] For example, the adoption of the language of economics by McCarthy and Zald (1977) and Zald and others (1987), who used such terms as movement "entrepreneurs," "movement industries," and "movement sectors," led some scholars who had been active in the movements of the 1960s to reject their research program in its entirety, partially out of distaste for what they mistakenly saw as a conservative bias.

This takes us back to the books under review and to a puzzle: Between the guiding assumptions of the collective action paradigm and those of the contentious politics school, there is an underlying homology, or at least a dovetailing. Both assume that potential actors mobilize not in response to raw grievances and discontents but as the result of the incentives and opportunities that surround them. While rational choice scholars find these incentives in individual calculations of cost and benefit, and political process scholars see the decision working through group processes and political opportunities, members of both schools explain variations in participation as the result of constraints and opportunities. Therefore, potentially creative intersections between the two approaches cry out to be explored.[21]

Where do these six studies take the dialogue between the choice-theoretic paradigm and political process approaches? Not very far. Tilly, who surveys a number of competing theoretical approaches to British contention, does not include rational choice among them. Andrain and Apter discuss the rational choice perspective but mainly in connection with elections (pp. 260ff) and appear to confound it with behaviorism. Jenkins and Klandermans do include in their reader the work of one group of rational choice researchers (Karl-Dieter Opp and his collaborators), but they barely mention the theory in their introduction. Elizabeth Perry mentions it, too (p. 37), but only to express approval of Michael Taylor's emphasis on community in his 1988 essay.

Of all our authors, only James White and Kriesi and others try to access the rational choice tradition empirically and theoretically. White uses rational choice perspectives to interpret his empirical findings but does so only inferentially. Lacking individual-level data, he operationalizes rationality in early modern Japan as interests, which he measures by proxy through urbanization, economic vulnerability, and productivity per capita (p. 233). These variables are lodged at several removes from the individual strategic decision making that most authors see as central to rational choice theory. Using these admittedly proxy measures, White finds "an extraordinary consistency between popular behavior and the assumption of popular rationality" (p. 234). Though these results are suggestive rather than definitive, White at least gives the rationality assumption a run for its money.[22]

White's findings point to one of the weaknesses of the political process model: Although it is better rooted empirically than work in the CA tradition, few authors in this tradition attempt to assess the strength of political opportunity variables against more traditional economic ones (e.g., inequality, inflation, unemployment). For example, Kriesi and others deal with "cleavage structures" as one of their defining variables, but these are deduced as national constants (chapter 1), which gives the authors no analytical leverage to relate changes in economic conditions to changes in political opportunity.[23]

Kriesi and colleagues criticize rational choice theory for leaving vague the thesis that social movements are rational decision makers who make strategic choices (p. 37). Stated in such vague terms, they argue, the thesis "does not constitute much of a bridge between political structures and movement action, since it leaves us not much wiser with regard to the ways in which political opportunity structures translate into costs and benefits at the individual level" (pp. 37–8). They suggest instead returning to Tilly's (1978) distinction among facilitation, repression, reform, and threat—in other words, a set of concrete opportunities and constraints derived from political structure which directly affect the costs and perceived benefits of collective action.

Why have students of contentious politics shied away from testing the powerful deductive logic of rational choice? It may partially result from the fact that they mostly employ aggregate protest data and have a penchant for structural analysis, while rational choice theorists focus on individuals and are less sensitive to the permutations of the structural and historical contexts that surround them. It may also relate to the greater mathematical inclination of practitioners of rational choice, compared to the richer empirical and historical resources of students of contentious politics.[24] Given the homologies between the two approaches and their obvious complementarities, however, more scholars should take up Lichbach's challenge and attempt to confront them.

CONTENTIOUS POLITICS, SOCIAL MOVEMENTS, AND POLITICS

This takes us to my final observations. Focusing on contentious politics, rather than on individual social movements, will allow us to link contention to conventional politics, to political alliances, and to the changing seasons of the political struggle more effectively than older studies of social movements or collective behavior. But, by the same token, it creates problems in differentiating social movements within the general range of contentious politics, as well as that of distinguishing the latter from politics in general.

Look, first, at the concept of political opportunity structure: Often considered a variable, it is really an aggregate of separate variables. By breaking it down into a small number of finite dimensions (e.g., the presence or absence of influential allies, the opening of possibilities for legal collective action, splits within or between elites, realignments in the party system),[25] the importance of opportunities in triggering movements can be operationalized and assessed and their changes related to conventional politics. But if opportunity structure is allowed to become a catch-all term for any interaction between a group and the state, or if the concept is specified post hoc, then we will end up with ad hoc analyses that border on descriptions. One of the priorities in the next wave of studies in this tradition must be to pin down opportunity structures in a more testable and widely accepted form.

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[21] For a preliminary attempt to do so, see McAdam, Tarrow, and Tilly 1996b.

[22] White also tries to assess opportunities for protest, which he measures as variations in shogunal control and the size of the local samurai population. He finds both interests and opportunities increase the propensity to protest, with opportunities slightly outweighing interests at county levels and the reverse occurring at the provincial level (p. 237).

[23] For a provocative cross-national analysis that relates opportunity structure to inequality in producing collective action, see Schock 1996.

[24] But this is not true of all rational choice practitioners. See, for example, Chong (1991), Golden (1996), and Tsebelis (1990), who combine comparative and historical materials with powerful deductive models.

[25] See Mcadam's defining essay on political opportunities in McAdam, McCarthy, and Zald (1996, chapter 1).

Second, consider the place of social movements within the broader field of contentious politics. Tilly, who comes closest to formulating this relationship theoretically, regards movements as only one form of collective action, a point on a typology consisting of the scope of action and the orientation to powerholders (1983). Other points on this continuum, for Tilly, would be rick burnings, forced illluminations, the pulling down of houses, the *charivari*, the strike, and the protest demonstration. But if movements are no more than a point on a continuum of types of collective action, are they observable events, in the same sense as some of these other actions? Then, what of movement organizations that *employ* these other forms? Can an observable event employ another observable event? Or is a social movement more than an observable event? I prefer to think that a movement is an actor or coalition of actors whose presence can be traced by observing the combination of collective actions which typify its interaction with its antagonists, allies, and publics but one that is not reducible to or comparable to a particular form of action.

None of our other authors seriously takes up the theoretical issue of the relationship between social movements and contentious politics in general. Andrain and Apter do not discuss it; White and Perry focus on different forms of contentious politics without raising the issue of the role of social movements within that universe of cases. Jenkins and Klandermans study social movements without specifying a broader universe of contentious politics. And Kriesi and others simply assume that the newspaper record of contentious public events in the areas of concern of the new social movements during the period studied in itself constitutes the record of the dynamic of the "new" movements of that period.

But are all the individual actions enumerated by Kriesi and colleagues—or, for that matter, by any of our other authors—necessarily "*movement* events"? Are there no contentious collective actions carried out by nonmovements? And are there no movement activities that are not captured by the record of contentious collective action? Authors writing in the "new" social movement tradition appear to think so (Melucci 1988); students of the women's movement point out that much of its work is done in private, or at least in "unobtrusive" forms of mobilization (Katzenstein 1990). There is a good deal to be said for both positions. Especially given the tendency for the "normalization" of protest in Western societies since the 1960s—in which parties, unions, interest groups, and temporary coalitions of local actors regularly employed contentious forms of politics—this poses a major conceptual problem for the event-based study of social movements and one that the next wave of studies will need to confront.

This leads to a final question, that of the relationship between contentious politics and politics in general.[26] As defined above, the study of contentious politics includes all situations in which actors make collective claims on other actors, claims which, if realized, would affect the actors' interests, when some government is somehow party to the claims. In these terms, wars, revolutions, rebellions, social movements, industrial conflict, feuds, riots, banditry, shaming ceremonies, and many more forms of collective struggle potentially qualify as contentious politics. Although, from time to time, a heroic synthesizer such as Kenneth Boulding (1962) has laid out a general theory of conflict, the study of contentious politics has not proceeded as a unified field. Instead, specialists in different kinds of political contention have created sui generis models of their subject matter, often ignoring powerful analogies or continuities with neighboring phenomena. As a result, each group of practitioners has emphasized a different set of concepts, theoretical issues, and comparisons.

These six studies attack the problem of synthesis empirically rather than theoretically. But the problem they raise is one of knowing what to *exclude* from the range of contentious politics. Are interest groups part of this population? Or only those interest groups which engage in noninstitutional actions? Or only interest groups *when* they engage in such actions? Conversely, do social movements engaging in legal actions, as so many do today, escape the boundaries of contentious politics? And what if, having completed a successful judicial challenge or failing to do so, they turn to more disruptive actions? Can we be content to retrieve actors in our net of political events only when they are anti-institutional and allow them to slip through it when they work within institutions?

Perhaps the solution is to focus on the strategic interactions between claims-makers and authorities whenever the claims made threaten some fundamental standing commitment of powerholders or other groups, regardless of the tactics used by claimants? But such an approach would cut against the grain of one of the main achievements of the new study of contentious politics: the methodological advance of focusing serially on contentious event histories. Studying movement actions regardless of whether they produce contentious clashes in public space would require a broader, less homogeneous, and immensely larger measurement strategy than the systematic enumeration and analysis of protest events. Practitioners of the new approach will have to decide which virtue they want to maximize, an inclusiveness that will combine serial data on protest with other kinds of information, or an exclusiveness that maximizes homogeneity and seriality at the risk of losing important information when movement actors work within institutions.

But these are problems for the next generation of studies of contentious politics. In these six books, the study of social movements has moved squarely back to its origins—the political struggle—from social psychology, organizational sociology, and public choice; it has produced a form of instrumentation that is a major advance on the idiosyncracies and dispersiveness of the case study; and in the concept of political opportunity structure, it has developed a theoretical pivot which advances comparison and produces hypotheses to explain movement emergence, dynamics, and outcomes. From an archipelago of books whose subjects range from eighteenth-century London to twentieth-century Shanghai, it would be hard to ask for more.

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[26] The following passage roughly follows the argumentation in McAdam, Tarrow, and Tilly 1996b.

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BOOK REVIEWS

Political Theory

The Sacred Fire of Liberty: James Madison and the Founding of the Federal Republic. By Lance Banning. Ithaca: Cornell University Press, 1995. 543p. $35.00 paper.

Vincent McGuire, *University of Colorado at Boulder*

Lance Banning, continuing his pursuit of the intellectual principles of the Founders, has given us a novel insight into what can only be called the odyssey of James Madison. Banning's singular contribution to the debate surrounding Madison's intellectual consistency is situating Madison within the disparate and often contradictory thinking of the founding generation. Much has been written about Madison. Was he a centralist, a states-righter, an inconsistent opportunist, or something else? Banning, with painstaking research, shows that, above all the debates, politics, and intellectual infighting of the period, Madison's overriding concern was maintaining the spirit of the Revolution.

Banning started out to write a biography of Madison but could not get past the contradictions between the conventional understandings of Madison and Madison's own thoughts and words. Professor Banning switched projects to an intellectual history when he attempted to answer the question What would Madison think about the way in which we perceive his thinking? Banning's conclusion is that the Madison we think we are familiar with is not a product of Madison's own words.

The crux of the book's argument, which proceeds chronologically, is that Madison was fully consistent throughout his career. The consistency hinges around Madison's drive to preserve the freedom of the Revolution as characterized by government by the consent of the people; this is the spirit of the Revolution. The primary problem that prevents us from fully understanding Madison is that he is usually seen as a centralist who discards the Articles in favor of the Federal Constitution centering power in the national government. Banning shows that Madison was at best a reluctant centralist. The centralist tendencies, greatly overblown by former analysts, are grounded in Madison's belief that the only way to preserve the freedom of the states was to make explicit and specific the national powers he believed were always inherent in the Articles of Confederation.

Madison believed the primary way in which to ensure the independence of the national government was to give it an independent source of income. But this is only part of the solution. The problem of the Articles for Madison was the ensured independence of the states. The ability of states to act as equals with the national government in national policymaking was the crucial fault in the Articles. The result of this disjunction with the powers the national government inherently obtained from the Articles would be the eventual collapse of the union at the expense of the states and the freedom attained through the Revolution. Thus, a new constitution, ratified by the people, not the states, is crucial. The centralizing of the new government was not a problem for Madison, because he had long believed that the powers of the national government were as much a matter of common sense as of fundamental law. In this reading, it is difficult to corner Madison too closely with, for example, Hamilton. The Banning reading of Madison places him squarely in line with Thomas Jefferson's thinking.

An oft-used piece of evidence demonstrating Madison's centralizing tendencies is his advocacy of a national veto, that is, the ability of the national government to veto state legislation. Banning moves the discussion by showing that the remainder of all power that is not national resides in the states, giving them a great deal of power. This assured freedom but could inhibit the national exercise of power. A national veto would not only protect the national government from the encroachments of the states but protect the other states from the encroachments of more powerful states. Indeed, Madison believed the veto would be used to prevent states from "taking actions inconsistent with their federal duties or infringing private rights" (p. 154). The end result would be the assurance of truly popular control of the government.

This is the essential contribution of *The Sacred Fire of Liberty*. Banning rejects the tendency among modern analysts to compartmentalize past thinkers by imposing "current scholarly conventions and contentions" (p. 368). The Republican versus Liberal debate does a disservice to understanding Madison. The inconsistencies and changes in thought are not Madison's but the product of modern assumptions.

Banning makes the argument that there are two primary faults in modern analysis that result in a misinterpretation of Madison's thinking. First is the "insistence on the absolute centrality of *Federalist* 10." Second is the lack of understanding of Madison's full appreciation of confederalism, that the states and national government would be sovereign in their particular universes. We know this, but Banning claims we do not fully appreciate the concept. In *Federalist* 10 Madison does not fully develop a particular problem of modern politics: that the tendency of a dangerous majority not to form also inhibits the coalescence of a sound majority. Thus, majority opinion, participation, had to be elicited and energized. Toward this end, Madison participated greatly in a search to develop majority opinion. His failure has led to the mistaken view that he missed the flaw of the constitution: the lack of participation.

Seen from this perspective, Madison's participation in the Virginia and Kentucky Resolutions is not a change of heart but a consistent consequence of confederal thinking. This leads to a final, and crucial, area of misreading of Madison: his break with the Washington administration and particularly Hamilton. In an appendix, Banning shows that the *Federalist Papers* never were, nor were they intended to be, of whole cloth. Banning dates Madison's "uneasiness" with Hamilton to 1783, significantly before the traditional break with the Federalist positions. Indeed, the progress of politics following the convention was not, and could not have been, foreseen by Madison. His association with certain beliefs is simply a mistake. Had Madison foreseen the interpretation and employment of the Constitution by Hamilton and others, Banning speculates, the Virginia delegation may have had a third nonsigner at the Constitutional Convention.

Justice as Impartiality. By Brian Barry. Oxford: Oxford University Press, 1995. 315p. $35.00.

Charles Larmore, *Columbia University*

Brian Barry's new book forms the second volume in a projected trilogy entitled *A Treatise on Social Justice*. It offers a systematic account of the liberal-democratic idea of justice as impartiality. Both those seeking an introduction to the contemporary debate about social justice and those with views of their own on the subject will profit from this book. Barry is an acute thinker with a real talent for getting at the heart of a philosophical position. He is also a vigorous writer, delightfully uninhibited in his criticisms of others, as when he writes of the "morally pathological universe" that David Gauthier occupies (p. 42) and the "social scientific illiteracy of someone like [Robert] Nozick, who is in these terms the equivalent of a pre-Copernican astronomer" (p. 214).

Principles of justice are impartial, Barry holds, if all those whom they are to bind can feel that they have done as well as they could reasonably hope—if, in other words, the principles constitute terms of agreement that no one can reasonably reject. As Barry acknowledges, this conception has received an influential formulation in the writings of the philosopher T. M. Scanlon. Each of the three parts of Barry's book is devoted to exploring a different aspect of it.

Part I aims to show the superiority of this ideal of impartiality to other accounts of justice. To the idea that justice consists in principles of mutual advantage, a conception stretching from Hobbes to the recent writings of Gauthier, Barry makes two familiar objections. First, justice so conceived is unstable: to avoid the costs of conflict, prudent people have good reason to agree to articles of peace limiting the pursuit of their own good, but they also have reason to break these rules when they see that their advantage would thus be best served. In addition, this conception violates an elementary requirement of justice, namely, the equitable treatment of the weak and immature, who have not the power to make it advantageous for others to respect them. The first difficulty can be removed, if one supposes (like Allan Gibbard) that a sense of fair play can move us to comply with rules of mutual benefit, even when taking advantage of others might procure us a greater good. But Barry rightly insists that this hybrid account of justice as "reciprocity" not only leaves intact the second difficulty, but it also creates a hodgepodge in which the criterion of justice (mutual advantage) and the motive of justice (fairness) pull in contrary directions. If we really are moved by a sense of fairness, why would we settle for rules of justice that instead are determined by the distribution of power?

Barry also argues that John Rawls's theory of justice, based on what people would agree to in an "original position," embodies an inadequate view of impartiality. His objection is that, being deprived of their conception of the good and of any individual knowledge behind a "veil of ignorance," the parties to Rawls's original position must all reason identically. As a result, Rawls fails, contrary to his intention, to "take seriously the plurality or distinctness of individuals," which is the essence of impartiality (p. 59). However, Barry's criticism seems to rest on misunderstanding. Any conception of justice as the object of reasonable agreement, even the Scanlonian formulation Barry favors, must have the feature that he finds objectionable. This is because people divided by different conceptions of the good must nonetheless share certain convictions if they are to agree upon rules by which they can live together. The reasoning that leads from that common ground to principles of justice must thus be identical for each of them. For there is little likelihood that people reasoning from different starting points would converge upon the same principles of justice; and, in addition, the political value of solidarity is best served by principles of justice that enjoy a common, public rationale, which citizens can affirm together.

In Part II, Barry looks at the relation between impartial justice and conceptions of the good. Against antiliberals such as Alasdair MacIntyre, Barry defends the position that this ideal of justice is indeed neutral with regard to conflicting views about human fulfillment. He also claims—this time against liberals such as Thomas Nagel and myself—that for principles of justice to rest thus on neutral ground, a certain scepticism about the validity of any of these conceptions of the good life is necessary. Otherwise, a person might argue that his view about the meaning of life should serve as the basis for society's major institutions, since it is one that every reasonable person should be prepared to accept. But the trouble with Barry's idea that citizens of a liberal democracy must regard their conceptions of the good life as less than certain is that the criteria for certainty and the question of what beliefs admit of certainty are things on which reasonable people disagree. The liberal ideal is to determine principles of justice on which reasonable people can agree despite their differences, and that is why I continue to believe it best to distance liberalism from scepticism. To my mind, even those with an unshakable faith in God have, like others, reason to look to secular foundations for the principles of justice, if they are committed to the norm of equal respect. Barry does not think this norm can justify basing principles of justice upon neutral ground. But it can, if understood as meaning that to treat persons as ends in themselves is to require that principles with which force may be used to ensure compliance—in other words, political principles—must be ones to which they also can reasonably agree. The basis for principles of justice that are neutral with regard to conflicting conceptions of the good must be in the end, not epistemological, but moral.

Part III responds to several objections to the ideal of impartiality, the most important being Bernard Williams's claim that this ideal undermines the ability to pursue with dedication those personal projects that alone give life meaning. We would have to stand ready to sacrifice those projects whenever the opportunity to promote a greater impartial good should present itself. Barry's reply is that, though unacceptable as a rule of conduct for all individual decision making, impartiality is valid as a principle for the regulation of social institutions. A good point. But Barry also formulates this distinction as one between first-order and second-order impartiality, and this has the implication, which he seems to accept (pp. 224–7), that impartiality, as a second-order principle against which more immediate considerations must be tested, is the supreme norm to which individual decisions have to defer. If so, then Williams's worry is not dispelled, for our personal commitments will still be forever at the mercy of what impartiality requires. Not a distinction between levels of principle, but rather between the circumstances where impartiality is relevant and where it is not, is the proper way out.

Really Existing Nationalisms: A Post-Communist View from Marx and Engels. By Erica Benner. Oxford: Clarendon Press, 1995. 266p. $55.00.

For Love of Country: An Essay on Patriotism and Nationalism. By Maurizio Viroli. Oxford: Clarendon Press, 1995. 206p. $29.95.

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The revival of interest in the political theory of nationalism provides both an opportunity and a good reason to look again at how earlier theorists have responded to questions of nationality. Both of the books under review represent excellent attempts to draw from the past intellectual resources to help us think about the problems of the present. Benner's is the more narrowly focused of the two. She reconsiders in depth the thinking of Marx and Engels on national questions, before showing, in a long concluding chapter, how contemporary treatments of these questions may be deepened by insights derived from the Marxian analysis. Viroli engages in a wide-ranging trawl through thinkers from Cicero to Mazzini to reconstruct a republican tradition of patriotism that celebrates love of country, but which identifies *country* with a regime of political liberty rather than with a culturally defined nation. Both books display an admirable willingness to combine historical scholarship with engagement in contemporary debates about national identity, national self-determination, and the treatment of cultural minorities.

Benner is chiefly concerned to dispel three myths about Marx and Engels: first, that they were irredeemably hostile to national identities, regarding these as illusions that would disappear in the course of the working-class struggle; second, that their explanations of nationalist movements were dictated by their general materialist conception of history; third, that they were disabled from laying down normative guidelines for judging nationalist claims by their adherence to a form of political realism that assessed all such claims according to whether or not they would help to bring closer the final goal of global communism. Through a comprehensive study of the original texts, she is able to show that Marx in his early writings sought to distance himself from the statist connotations of the German concept of nationality (as found in the writings of Hegel and others), replacing this, however, with a democratic idea of "the nation" as a community of people distinct from the state apparatus, which might in due course form the basis of a nonexploitative self-governing society. In exploring Marx and Engels's commentaries on contemporary political movements and events, she demonstrates (in line with much recent scholarship) that the two authors did not treat classes as bearers of fixed historical roles, but as actors having to make strategic decisions (for instance in their choice of class allies). Nationalist movements were thus scrutinized according to whether they did or did not promote programs of political and social reform, as well as according to their bases of class support. This in turn allowed Marx and Engels to make recognizably ethical judgements about different species of nationalism, depending on whether they supported or sought to repress liberation movements in other countries, whether their social policies were progressive, and whether what was aimed at was a viable form of national self-determination.

Benner does not claim that Marx's analysis, with its heavy emphasis on class struggle, can be used wholesale in understanding contemporary nationalism. But she does argue very plausibly that attention to the ways in which nationalist movements are fueled by the particular material and psychic needs of the participants can help distance us from two perspectives that she calls "methodological nationalism" and "liberal post-nationalism." The first of these assumes that the perennial drawing and mobilizing power of national identities is by itself sufficient to explain nationalist movements from the most moderate to the most ferocious; in other words, it discards any reference to the economic or political interests of particular social groups. The second view holds that liberal political regimes supersede nationalism and that once liberal rights and democratic institutions are in place, questions of personal identity—group of national—are no longer politically significant. Drawing on recent experience in eastern Europe, she is able to show how misguided both of these perspectives have proved to be. For a young scholar this is a remarkably subtle and mature piece of work, and it must now stand as the best analysis yet offered of the Marxian understanding of nationalism.

Viroli's main aim is to invoke patriotism as an antidote to nationalism. He remarks that the two are often confused, but then seeks to show that patriotism, understood as loyalty to a particular set of laws and institutions that guarantee political liberty, is quite distinct from nationalism, which appeals to the idea of the nation as a distinct, culturally homogeneous community. The latter, he argues, leads to external hostility and intolerance toward minority groups who do not share the national culture. Viroli's tracing of the republican tradition of patriotism is a remarkable feat, ranging across several countries and many centuries, and he is able to show convincingly that the *patria, patrie*, country, etc. to which writers in this tradition devote themselves is neither a physical place nor a cultural defined people, but a political idea.

There are, I think, two difficulties with this analysis. Viroli acknowledges that patriotism always involves loyalty to a particular *patria* and indeed must do so if it is to perform the same political-psychological functions as nationalism. As he puts it, "I do not mean love of the republic in general or attachment to an impersonal republic based on universal values of liberty or justice. I mean the attachment to a particular republic with its particular way of living in freedom" (p. 13). But then we must ask what differentiates one republic and one way of living from another if it is not such things as a shared history or cultural values. And if it is indeed such factors that gives each person's patriotism its particular focus, then the line between patriotism and democratic forms of nationalism becomes more difficult to draw.

The second difficulty is that Viroli describes, but does not attempt to explain, the engulfing of patriotism by nationalism in the nineteenth century. One part of this story must surely be the irruption onto the political stage of large numbers of people who had previously been excluded from it. Whereas the older republican tradition had tacitly appealed to a political elite of active citizens, nationalism helped to form an inclusive political community from people divided by attributes such as class and religion. Since that is still our predicament today, it may seem that a direct attempt to get back to republican patriotism is anachronistic: we need the cement of a common culture to underpin our democratic politics.

Viroli comes close to acknowledging this necessity toward the end of his book when, in a comparison between the Italian theorist Gian Enrico Rusconi and Jürgen Habermas, he argues that we need to find a middle way between Rusconi's appeal to national identity as a support for civic virtue and Habermas's purely political "constitutional patriotism"; the latter's reliance on universalistic political values is insufficient. Benner too ends by claiming that we must find a third way between ethnic and civic nationalism—the former leading to intolerance and repression of minorities, the latter insufficiently responsive to those concerns about personal identity that nationalism has traditionally met. Neither author dwells at length on the problems posed for moderate forms of nationalism by the efflorescence in recent years of ethnic and other group identities in the liberal democracies: here, perhaps, neither the republican focus on the citizen with undivided loyalties nor the Marxian exploration of nationality and class gives us much immediate help. But to say that the conceptual frameworks of these two books cannot give us everything that we need in order to think clearly about nationalism is in no way to diminish the very considerable intellectual achievement that each represents.

Theories of Tyranny from Plato to Arendt. By Roger Boesche. University Park: Pennsylvania State University Press, 1996. 494p. $65.00 cloth, $25.00 paper.

Larry Arnhart, *Northern Illinois University*

This book surveys the idea of tyranny in the history of political theory. Moving in chronological order, Boesche offers extensive commentary on how tyranny has been analyzed by a dozen political theorists: Plato, Aristotle, Tacitus, Machiavelli, Montesquieu, Tocqueville, Marx, Freud, Weber, Fromm, Neumann, and Arendt. (It is hard to understand why Boesche does not include Xenophon or Locke.) The intellectual and historical diversity represented by these authors suggests an obvious question. Does tyranny from Greek antiquity to the present have any enduring meaning? Although Boesche denies that there is any "timeless idea of tyranny," the major thesis of his book is that "there is enough continuity in the phenomenon of tyranny to call it a perennial problem" (pp. 13, 21). But in explaining "the perennial problem of tyranny," he takes two incompatible positions. One position, which he adopts from Aristotle, seems persuasive to me. Another position, which he adopts from Marx, Weber, Freud, and Arendt, seems less persuasive.

Aristotle defines tyranny as the arbitrary rule of one person for his own interest and against the common interest of his subjects. Tyranny is unnatural, therefore, because it frustrates the natural human desire to be free from exploitative rule. Although Aristotle explains the many techniques that tyrannts can use to preserve their tyranny, he makes it clear that tyrannies tend to be the least durable of regimes. The more durable tyrannies are usually those in which the tyrant has so moderated his tyranny that he appears to be ruling for the common interest of the people. Much of Boesche's book confirms the enduring wisdom of Aristotle's analysis of tyranny, with its emphasis on the natural vulnerability of the tyrant (pp. 82–3, 411, 419, 469–70, 475). One of the most instructive parts of the book is Boesche's development of Leonard Shapiro's argument that ancient authors such as Aristotle could help us explain totalitarian regimes as twentieth-century variants of tyranny.

But despite his respect for this Aristotelian position, Boesche also adopts a contrary position that he sees emerging first in Tocqueville. Boesche thinks the modern world faces a new kind of tyranny—"tyranny without an identifiable tyrant"—that is far more pervasive and powerful than the traditional form of tyranny known to Aristotle. Boesche thinks we need to expand the definition of tyranny so that we say that "tyranny is the form of government that least fulfills human needs" (p. 458). Following Abraham Maslow, Boesche thinks there is a hierarchy of human needs that includes not only the basic needs for survival and physical security but also higher needs for "self-actualization." Rulers who use fear and violence to exploit their subjects can be condemned as tyrants who frustrate basic human needs. But even governments that apparently secure the safety and happiness of their people should be recognized as tyrannical if they promote a spirit of bourgeois acquisitiveness that distracts people from the political participation that satisfies their highest needs.

Boesche agrees with Arendt that the highest expression of human freedom is in a participatory democracy based on "the direct participation of every citizen in the affairs of the country" (p. 430). In modern liberal democracies, however, most citizens participate only indirectly and irregularly in government through the mechanisms of electoral representation. Direct participation in government is dominated by professional politicians and bureaucratic administrators, which leaves most citizens free to devote their lives to the private economic and social activities of family and workplace. Although most people in a liberal democracy are happy with this bourgeois way of life, their failure to participate in "public space" makes this a seductive form of tyranny. This is the most dangerous kind of tyranny precisely because people willingly choose it and thus think they are truly free.

In this "tyranny without a tyrant," Boesche believes, not only are the people in a liberal democracy excluded from political life, as shown best by Arendt, but their private lives are regimented by the discipline of a capitalist economy and bureaucratic administration, as shown best by Marx and Weber. This tyrannical repression of human needs extends even to the internal order of the human soul. Freud saw that "an internalized tyranny imposed on each of us by a harsh superego is the primary means by which civilization manages to check both our sexual and our aggressive instincts." This is the most powerful tyranny of all. "While we can at least imagine slaying Roman emperors or toppling French monarchs, what do we do when we have been taught to tyrannize over ourselves?" (p. 320). Once this invisible tyranny has been internalized in our souls, we pass it on to others—in our families and in our social circles of friends and neighbors—without realizing what we are doing.

At this point, Boesche's expansive view of tyranny becomes implausible. Aristotle understood the importance of a public-spirited political life as well as anyone. And yet he did not think this required a participatory democracy. In fact, he thought the best form of democracy was one in which the multitude of citizens choose and audit public officials but do not rule directly. Most citizens have no higher political ambition than this. Those who do wish to rule directly can seek election to public office, and any potentially tyrannical ambition they might have will be moderated by the need to win election by the people. The system of representative democracy adopted by the American founders is a modern version of this Aristotelian proposal for moderate democracy. Such a system seems to me (and other proponents of liberal democracy) to be a sensible response to the variability of the political nature of human beings—most people desiring only a limited control over those who rule them and a few people desiring direct participation in rule to satisfy their greater political ambition. To suggest, as Boesche does, that anything less than direct participation in politics by all citizens constitutes tyranny is a perverse denial of common sense. It is just as perverse to claim that all liberal democracies are tyrannies because of the "internalized tyranny" of the superego or because workers in a capitalist economy must report to work on time to keep their jobs.

As a concession to common sense, Boesche implies in one passage that the kind of tyranny that would conform to Aristotle's definition should be ranked as "more tyrannical" than the tyranny of liberal democracy (p. 280). But this would contradict Arendt's claim that "tyranny without a tyrant" is "clearly the most tyrannical of all" (p. 443).

Despite my disagreements with some of the arguments, I recognize this book as a great achievement in the scholarship of political philosophy. Anyone who wants to think deeply about the meaning of tyranny must read it.

Care, Gender, and Justice. By Diemut Bubeck. New York: Oxford University Press, 1995. 281p. $21.00.

Wendy Gunther-Canada, *University of Alabama at Birmingham*

In this provocative study Diemut Bubeck examines the exploitation of women as caregivers in a gendered society. This work's great strength lies in its materialist approach to the question of women's work as *carers* (Bubeck's term), providing a mechanism for critically analyzing how the sexual division of labor in society results in the unjust distribution of the burdens and benefits of care between men and women. *Care* is here defined as an action in response to a need that an individual could not meet alone. This focus on care as action rather than as attitude differentiates the author's treatment of care from other recent feminist theorizing about the ethic of care, most notably the works of Noddings, Held, and Tronto. Her theory of need as dependence highlights the powerlessness of all human beings at some point in their life cycle and calls into question the model that places the autonomous adult male at the center of mainstream political theory. In *Care, Gender, and Justice*, Bubeck argues that earlier care theorists have not satisfactorily explained women's exploitation as caregivers. Her materialist analysis of care exposes women's vulnerability to exploitation as caregivers and thus serves to caution feminists that they should examine larger questions of social justice before they embrace an ethic of care.

Bubeck draws on scholarship associated with both the domestic labor debate and the ethic of care vs. the ethic of right and justice controversy. Her examination provides an immanent critique of the sexual division of labor, which she links to a larger discussion of gender and distributive justice. At issue in her analysis is the question of why it is overwhelmingly women who take on the burden of care in society. "The most pressing question emerging about care, given its peculiar features, especially the material disadvantage involved in engaging in unpaid care, is why it is mostly women who do it" (p. 147). Bubeck differentiates between care and other types of work that men and women engage in, such as wage labor and paid/unpaid service. In doing so, she is able to define more closely what is unique about care and to demonstrate what specifically is exploitative about women's work as carers. For Bubeck, women's exploitation as caregivers consists of the fact that by acting as unpaid carers, they forgo other activities that could increase their material well-being and provide other social benefits to themselves.

To make her case, Bubeck employs what she terms an "exploitation story" as a heuristic device to explore the multiple dimensions of women's exploitation as carers. Through this mechanism she argues that the production model used by orthodox Marxists within the domestic labor debate effectively ignored women's exploitation as caregivers within the domestic sphere. Yet Bubeck suggests that feminists can employ a materialist approach to the analysis of women's work as carers without getting lost in the labyrinth of orthodoxy. An example of what she terms a "creative use" of materialist categories is Christine Delphy and Diana Leonard's argument about women's exploitation as wives. However, Bubeck contends that their exploitation story cannot fully explain women's exploitation as carers because it is based on an outdated model of domestic production that elides how all men—not just husbands—benefit from the work women do as caregivers.

It is this benefit to men as a whole that Bubeck elaborates in her own exploitation story. Central to this story is the circle of care. "The exploitative mechanism . . . is what I have called circle of care, an interlocking set of constraints and practices that channels women into doing the bulk of care that needs to be done in any society" (p. 181). The circle of care is a complex system of interconnected psychological, sociological, and political attitudes based on gender stereotypes about proper sex roles that predisposes women to take on the heavy burden of care and service to partners, children, and family. The strength of this concept is that it takes into account the great variability in women's social locations and personal histories, while providing a compelling explanation of why women as a group are exploited as caregivers. Bubeck argues that the constraints placed upon women by the circle of care have not been sufficiently examined by theorists of the ethic of care. Here she targets the work of care theorist Nell Noddings, claiming that Noddings's idealist formulation of the ethic of care poses a great threat to women as carers. Bubeck asserts that Noddings's *one-care* is especially vulnerable to the exploitative elements of caregiving in a gendered society. Just as women's vulnerability as the primary caregivers within families led Okin to question the treatment of gender and the family within mainstream political theory, it is women's particular vulnerability as carers in society that leads Bubeck to question the adequacy of an ethic of care to provide social justice for women.

Diemut Bubeck's critique of care theory raises some troubling questions for feminist political theorists. While many feminists have been attracted to the ethic of care because it allows them to speak with a different voice about gender, morality, and society, Bubeck argues that without careful considerations of justice feminists are in danger of articulating an ethic that makes women vulnerable to exploitation as caregivers. Indeed, it is just these considerations of gender and distributive justice that make this book such an important contribution to feminist theory. The relevance of her analysis to social policy is that in an era of federal budget cutting the rhetoric of family values obscures a transfer of the material burden of care from the public to the private sphere. Contrary to this trend, Bubeck suggests that the state must become more involved, not less, in the just distribution of resources for caregivers as well as those in need if the exploitation of women as carers is to end. However, most of her observations about these policy questions are confined to a running argument within the footnotes. Given the political importance of these policy issues, I would have appreciated a greater exploration of the relationship between private and public care within the body of the text. Diemut Bubeck's analysis of the social justice of care should provoke greater discussion of the constraints on women's participation as citizens as well as a reevaluation of women's service to the state as caregivers.

Classical Liberalism: The Unvanquished Ideal. By David Conway. New York: St. Martin's Press, 1995. 150p. $65.00.

Liberty for the Twentieth-first Century: Contemporary Libertarian Thought. Edited by Tibor R. Machan and Douglas B. Rasmussen. Lanham, MD: Rowman and Littlefield, 1995. 416p. $67.50 cloth. $26.95 paper.

James M. Glass, *University of Maryland*

The two books under review are arguments in support of classical liberal perspectives. David Conway engages in a philosophical debate with modern liberals, communitarians, and conservatives; the contributors to the volume edited by

Tibor R. Machan and Douglas B. Rasmussen provide ideological justifications for an unrestrained individualism and a market society unfettered by government regulation.

In his defense of classical liberalism, Conway sees the modern liberal as favoring government regulation and state welfare spending; modern liberalism's defense of egalitarianism, in his view, has gone too far. Affirmative action diminishes the status of merit; welfare saps individual initiative; the pursuit of utilitarian justice puts personal liberty at the bottom of the ethics ladder; and the redistribution of social assets (taxes) "fails to treat with respect" the income of working and earning citizens (p. 41). John Rawls's defense of justice (in *A Theory of Justice*) penalizes talent, merit, hard work, and natural abilities. In a system of natural liberty, those with better natural endowments should, by virtue of their talent, be entitled to enjoy better life-prospects than those less favorably endowed (p. 33). For Conway, benefits gained from "liberty" (read: accumulated property) far outweigh the demands imposed by the redistributive consequences of equality. To tax is to break social trust.

Conway holds that the concept of equality should be taken seriously only in its strictly juridical context as "equality before the law." Every citizen is "equal" before the bar of justice; each has an equal right to bring a claim for redress; each has an equal right to demand restitution for wrongs suffered. Equality, as an economic concept or redistributive principle, becomes for Conway an assault on liberty. Somehow the legal system magically guarantees that the least fortunate among us, in terms of the ability to amass wealth, will not suffer too much. Conway assigns considerable beneficence to the law, as if the law by its very nature serves "justice"; as if law has nothing to do with human passion, desire, greed, rage, vengeance, hostility, bias, and prejudice; as if the bar of justice automatically assures fairness; and as if those who control legislation intuitively will respond to social injustice with enthusiasm. The messy human and motivational foundations of "the law," its tendency to protect interest and power, never seem to affect Conway's analysis.

Everyone benefits, Conway maintains, from the absence of barriers to unlimited possessiveness; therefore, the modern liberal fails to understand how the entire society gains from maximum accumulation and the commitment *not* to redistribute wealth. To "hold, and transfer at will, assets, which at the time of the appropriation are not anyone else's"—in other words, taxation—represents, in Conway's view, an assault on individual rights (p. 41). It stretches credibility, however, to argue that a multinational shoe or clothing manufacturer searching the globe for 38-cents-an-hour workers engages in economic activity supporting a broader social interest in helping people keep what they earn. How kindly do American workers look on corporations that, to maximize profits, close down factories, transfer operations to low-wage countries, and create unemployment and depression in local communities?

The assumption that private property assures liberty, without regard to economic, psychological, or social consequences, drives Conway's support of classical liberalism. But what does history teach in this regard? Who benefits from unregulated markets? What groups thrived under the regime of classical liberalism? Marx's description, for example, of the factory system in mid-nineteenth century England hardly suggests that "personal liberty" and "private property" were enjoyed by anyone but a very few ruthless possessors. In fact, the majority of the population, their labor, and their anguish allowed property and liberty to flourish for the minuscule percentage who owned the labor of others. To speak of this governing system as "justice" is an unabashed defense of the few over the many and a direct assault on democratic forms of political and economic organization.

It is extraordinary how this book ignores political and economic history, the history of legal claims against employers who discriminate, and the utter indifference of classical liberalism toward women, their rights and economic status, the poor, and the mentally ill. To be believable, theorizing needs at least some reference to history and practice; otherwise, theory becomes an exercise in illusory projection concerned only with demonstrating the truth of its hermetically sealed assumptions.

Communitarian theorists like Michael Sandel and Alasdair MacIntyre, Conway argues, distort (as do modern liberals) the classical liberal position regarding the concept of social good. Liberals do not argue for the unleashing of individual will and aggression against the good of the community: "Communitarians grossly exaggerate the magnitude of the measure of individualism as licensed by the liberal polity" (p. 71). Again, history would be a reliable guide here. C. B. Macpherson's (1962) *The Political Theory of Possessive Individualism* argues that individualism as "licensed" by classical liberalism is not only intent on possessing but is also savagely greedy, indifferent to suffering, and fixated on profit and accumulation, with little concern given to the physical, moral, or psychological welfare of those who labor for wages. Conway claims that the liberal polity supports "community in the full-blooded sense of the term" (p. 7), but community for whom? Who benefits from "full-bloodedness," whatever that ambiguous term means? Is it landholders, biologically defined groups, property owners (capitalists), party entrepreneurs?

What happens to those excluded from this "full-blooded" universe, namely, the poor, women, minorities, those suffering from debilitating emotional and psychological disorders? Do these people possess or have access to "community" in the sense intended by theorists like Locke and Smith?

For Alasdair MacIntyre (1984, *After Virtue*), a noted communitarian theorist, Aristotelian virtues fall away in the environment of classical liberalism. Conway, however, argues the opposite: The classical liberal encouraged the development of individual virtue and saw society as contributing to the moral welfare of the individual. MacIntyre, however, seems to have the better of this argument. Classical liberalism, in its fetish for property, its paranoia regarding threats to wealth and accumulation, and its assignment of moral development to the private sphere, represents an attack on Aristotelian ethics and the concept of polity or community.

It is a distortion of Aristotelian philosophy to argue that the Aristotelian lexicon of virtue and its relation to moral character encouraged appropriating the labor of others, storing up profits and defining the ends of human existence as the accumulation of wealth. Nothing in Aristotelian ethics suggests that moral development arises from greed, domination, unlimited self-entitlement, and endless aggrandizement. To see these "values" as part of the Aristotelian schema of ethics is to me a loose reading of Aristotelian political and ethical philosophy.

The book concludes (after a discussion of conservative critics) with the observation that classical liberalism "enables its members to attain greater well-being or happiness than does any other societal form" (p. 135). Yet no empirical evidence or historical analysis is offered in support of this unqualified assertion. Conway speaks of classical liberalism as the "unvanquished ideal"; it is the subtitle of his book, but unvanquished for whom? Rousseau and Marx considered classical liberalism to be a brutalizing form of economic organization, inducing massive social divisiveness and inequalities. Modern liberal theorists such as John Dewey and John Rawls describe significant practical and human deficiencies in this so-called ideal. Freud, subsequent generations of psychoanalytic theorists like Herbert Marcuse and Erich Fromm, and the critiques of the Frankfurt School have all noted serious psychological contradictions provoked by an economic system advocating unlimited accumulation and insensitivity to the victims of appropriation and greed.

Disregard for those victimized by free market forces runs through the volume of essays edited by Tibor R. Machan and Douglas B. Rasmussen. *Liberty for the Twentieth-first Century* is a wide-ranging philosophical defense of radical libertarianism. The contributors champion not democracy and participatory institutions but an unrestrained individualism, which includes the liberty to buy and sell without constraint, to possess without limit, to defend oneself without interference by the state, and to be free *not* to acknowledge the suffering of the least fortunate.

The essays are of varied quality. One of the most provocative, Jan Narveson's "Contracting for Liberty," could serve as the ideological guidepost in a G. Gordon Liddy handbook of "decisive" action. Narveson, an unabashed admirer of Thomas Hobbes, suggests a shoot-from-the-hip, straight-out defense of if it's yours, never let anyone mess with your property; take no prisoners; and the poor deserve to be poor, so refuse to waste energy or resources on defective human beings. Narveson's theoretical assertions embrace a rabid individualism, placing property and force at the center of the human project. Narveson, who demonstrates none of Conway's subtlety in arguing the classical liberal position, directs his contempt at anything or anyone that might help those who lack the skills, luck, or status to compete successfully in a stark, paranoid universe driven by power and property.

Ideology permeates these essays. Some contributors pride themselves on their truculence, hardness, tough-mindedness, a self-congratulatory insensitivity to anything that might, in their view, suggest equality or justice. Egalitarianism and democracy threaten property rights; libertarians hate democracy, since a democratic politics supports a measure of redistribution and the assignment of taxes, takes a skeptical and guarded view of authority and order, and places a premium on justice over the liberty of the few. Or as Narveson succinctly puts it: "The best politics, in short, is no politics" (p. 38), which was exactly Hobbes's position regarding the power of the sovereign to impose order. Politics, in this view of the world, reeks of negotiation, compromise, empathy for the least well-off, oppressive regulation, and restraints on the accumulation of wealth.

Repeatedly, the Lockean notion of "self-ownership" appears in these narratives: the individual possesses an inalienable right to sell the body's labor, to put up for bid the "property" contained in the self (that is, the self's laboring value and to sell labor for wages). Macpherson took a dim view of this Lockean postulate, since self property in the context of emerging capitalism meant essentially a cheap supply of labor. Self-ownership for the vast majority of workers had the *practical* effect of placing the "body" in a miserable wage environment that could barely sustain subsistence. Obviously, today, the Honduran garment worker or the displaced button stitcher in Alabama has a different concept of self property than the multinational distributor purchasing finished goods from international contract suppliers. But for these essays on "liberty" such differences and distinctions contain little merit for the ideology's logic.

Steven Yates, in "Righting Civil Wrongs," lists the major elements of what he calls the "philosophy of social spontaneity," or libertarianism: the centrality of the individual (understood as individual as possessor); a moral environment of free will (no regulation of the freedom to accumulate); personal responsibility (if you are poor and fail at accumulation, it is your fault and not the fault of the system; libertarianism recognizes not victims, only rational actors attempting to maximize their self-interest); limited government (no "group entitlements"; it is immoral to make a claim on government for redistribution through taxation); voluntarism (government should not be in the business of regulation); the private sector upholding social welfare assistance (private associations, not government, should be the primary charity-givers to the poor and disabled).

The essays in *Liberty for the Twentieth-first Century*, motivated by ideological commitments of the far right, never ask what actually happened when these libertarian principles governed the political and economic environment. In this extreme ahistoricity, the arguments take on a peculiar narcissistic quality, with their guiding premises confirmed not by evidence but by abstract logic tested only against themselves. No experiential context brackets theory construction; no effort at historical measurement demonstrates the truth or lack of it in these theorists' political and economic assertions. In fact, when libertarian theory, in the days of emerging capitalism, dominated the thinking and actions of business and government, it was at the expense of enormous suffering throughout the population. That has been well documented by economic and social historians describing actual conditions of life during the early phases of industrialization and the emergence of capitalist economic organization and distribution.

Theory, particularly in the Machan and Rasmussen volume, is consistent with the reasoning of jurists like Antonin Scalia and Clarence Thomas, and economists advocating unrestrained free market exchange, who see capitalist enterprise as a winner-take-all contest of unlimited accumulation. In their obsession with "individualism" (or, in effect, Macpherson's possessive individualism), these essays exhibit no sensitivity to the *cruelty* of the market, to the mess that markets make out of those who end up its victims.

These books, then, in their passion for theorists of the past, make serious statements about the organization of contemporary politics, the pursuit of property, the use of force, the role of minorities, the status of the welfare state, and empathy toward the less well-off. Without reference to history, with no interest in the psychological effects of "liberty" on those who fail in Adam Smith's "race for wealth and honors," both volumes speak to, as Conway put it, an "Ideal." But the problem with ideals is that they can readily disintegrate into delusions in which truth defines itself through a closed logical system and not the refractory mirror of history.

The Two Faces of Political Apathy. By Tom DeLuca. Philadelphia: Temple University Press, 1995. 294p. $49.95 cloth, $18.95 paper.

Philip Green, *Smith College*

As its title reveals, this book is a well-deserved hommage to Peter Bachrach, who did so much to bring about the 1960s revival of what DeLuca calls, in a chapter on Bachrach and E. E. Schattschneider, "plain democratic theory." The title also locates this book as a contribution to, and a kind of summing up of, the tradition of empirical studies of power and participation inaugurated four decades ago by the elite-oriented works of Floyd Hunter (*Community Power Structure,* 1953) and C. Wright Mills (*The Power Elite,* 1956); and the pluralist countercritiques of, most prominently, Robert Dahl (*Who Governs?,* 1961 and *Modern Political Analysis,* 1963) and Nelson Polsby (*Community Power & Political Theory,* 1963; 1980).

The pluralist challenge depended on, inter alia, a justification of *non*participation as voluntary and rational. This was provided by Dahl's division of political actors into what seemed to be inherent personality types (*homo politicus* and *homo civicus,* who could be mobilized into political life but was not essentially political) and also by the argument in Berelson et al.'s *Voting* (1954) that widespread apathy was both rational and system-preserving. In turn, within the theoretical episteme of empirical theory, the pluralist challenge was taken up by E. E. Schattschneider (*The Semi-Sovereign People,* 1960) and, perhaps most famously, by Peter Bachrach and Morton Baratz's instant classic, "The Two Faces of Power" (*American Political Science Review,* December 1962).

Bachrach and Baratz argued that the pluralists only looked at power in its overt guise as decision making and missed its hidden face, "agenda setting," which led to what could properly be called "nondecision-making." But even given this last notion, it still remained to be explained why so many people do not participate at any level when, in principle, the system is open to their political endeavor at least in the decision-making phase. Here, they suggested that nonparticipation of this sort is best explained by a realistic assessment of its likely futility, an explanation attacked by Polsby, with some justice, as question-begging. Nondecision-making needed a better grounding and was therefore explored theoretically and demonstrated empirically in, for example, Bachrach and Baratz's *Power and Poverty* (1970) and, later, Matthew Crenson's *The Un-Politics of Air Pollution* (1971). The next step was to argue that social hierarchy itself (e.g., the presence of a working class) generates nonparticipation, and this step was taken in works such as Steven Lukes's *Power: A Radical View* (1974), John Gaventa's *Power and Powerlessness: Quiescence and Rebellion in an Appalachian Valley* (1980), and Michael Parenti's *Power and the Powerless* (1978), all of which added the concept of structured domination or oppression to the explanatory field.

As DeLuca says, the argument for apathy strikes "an odd tone on the democratic ear"; his purpose, contrarily, is to "suggest a way to approach the problems of nonparticipation and political apathy that also serves to overcome both" (p. 2). The first part of the book is historical and covers familiar ground; it is a good and useful summary, though only those unfamiliar with the notion that the Founding Fathers (as well as many later distinguished American social theorists) were not populist democrats will find it original. Similarly, a section on "The Science of Liberal Politics" echoes the analysis of Bachrach's earlier *The Theory of Democratic Elitism* (1967; now sadly out of print). DeLuca's most original contribution is contained in a section titled "Explanations of Nonparticipation," in which he both summarizes and critiques the most well-known (what are better called) apologias for this phenomenon, from Berelson on. There are excellent discussions of *Voting,* whose authors are "trapped by their view that the American polity is the functional equivalent of modern democracy itself" (p. 80), a view that ineluctably turns social science into ideology; of Samuel Huntington, in whose hands "democracy becomes a tool technocracy uses to legitimate less democracy" (p. 87); of Dahl, who still "insufficiently appreciates the relationship between political participation, discovering what is truly in one's interest, and freedom, and rates too low within democratic discourse the importance of the element of responsibility" (p. 97) (though DeLuca also acknowledges the "healthy tension" in Dahl's more recent work); and of rational choice accounts of apathy (e.g., William Riker's) that by decomposing political reality into units of arbitrary individual choice leave the rational choice theorist without a leg to stand on for his own commitment to "democracy."

On the far left of the political spectrum, in the analyses of C. Wright Mills and Herbert Marcuse, apathy is conceived of as neither constitutive nor rationally chosen by discrete individuals, but rather as a structural social condition. DeLuca calls this "absolute political alienation," but also notes the similarity between the two apparently disparate approaches: neither can theorize the transformation of apathy into mobilization (Dahl's effort being an example of purely post hoc explanation). Moreover—and here DeLuca makes his most original contribution to the standard literature—even critics of "the liberal science of politics [Bachrach, Mills, Marcuse] fall short most when it comes to questions of how racial and gender disadvantages serve to depoliticize" (p. 163). As to race, he points out (what cannot be reiterated too often) that the end of de jure discrimination, which in principle would appear to have left black Americans in the same relationship to the polity as whites, in fact coincided with a closing down of both economic and political opportunities. This was depoliticization not by choice but by unyielding circumstances. And as to gender, he shows how feminist critiques of the public/private distinction serve to extend the problem of agenda-setting beyond the arena of policymaking that Bachrach and Baratz analyzed, so that some portion of the depoliticization of women can be seen as a response to the masculinization of politics itself. (It is instructive that the word *women* doesn't appear in the index of any of the basic works of "liberal social science," such as *Who Governs?*). As Gaventa and Parenti have demonstrated (unaccountably, neither is mentioned here), political quiescence is intimately related to the nature of structured, constitutive social hierarchies. DeLuca is the first commentator to relate this demonstration directly to the classical literature of apathy.

The final portion of this book, uniquely for the empirical theory tradition, is about "overcoming apathy." DeLuca first points out that reform proposals intended to overcome systemic constraints such as mass apathy are inherently paradoxical in that, as he nicely puts it (p. 233), they "overfly the real power (and crash)." Instead, and most provocatively, he argues for making "the ideal of *real political equality*" itself a political tool to "widen the scope of conflict, increase political participation, and thereby create political institutions capable of protecting political equality through widened participation" (p. 237). It's not clear that this approach will make fundamental change any more likely, but it does have the clear theoretical virtue of putting the horse (changing mass expectations and desires) before the cart (trying to reform structures). All in all, this is an excellent contribution and should be read by everyone with an interest in democratic theory.

Discourses of Power: From Hobbes to Foucault. By Barry Hindess. Oxford, UK, and Cambridge, MA: Blackwell Publishers, 1995. 184p. $49.95 cloth, $19.95 paper.

Jacqueline Stevens, *University of Michigan*

In *Discourses of Power* Barry Hindess characterizes two dominant conceptions of power in "Western political thought in the modern period"—the "capacity to act" and the "right to act" (p. 1)—suggests some variants of each, and then outlines Michel Foucault's alternative view of disciplinary and discursive forms of power. Chapters are devoted to Hobbes, Locke, Lukes, and Foucault, with attention to internecine differences among critical theorists appearing in the review of Lukes's arguments.

Hindess does a nice job of identifying some major contrasts among the theorists he discusses, but I am not clear on whom Hindess has in mind as his audience. Before proceeding, I should mention my own allergy to a certain kind of political theory text that simplifies rather than complicates and that ends with the engaging Big Question with which it might have been far more interesting to begin. Publishers must believe there is an audience for these works or they would not generate them at their current rate. What Hindess provides is essentially a Cliff Notes version of certain strands in political theory—Modern Writers on the Theme of Power—summaries redundant (if not oversimplified) for those who have read the texts he discusses and of doubtful utility for those who have not. These sketches are not simply place keepers to guide us through Hindess's own project on power, but are the trail and the end at the same time. Again, this is my own feeling on such volumes, and if others who teach courses on, say, "Power" feel it would be useful to assign a book that provides synopses of this concept in works by Hobbes, Locke, Lukes, and Foucault, then this is their book.

Here are some of the questions with which Hindess left me. First, why is power the organizing concept for his study, and not authority or legitimacy or sovereignty, for instance? My question is not to suggest that any of these would have been more fruitful to investigate than power, only that aside from his initial point that definitions of power vary, the author does not clarify for the reader why this particular contested concept is worthy of examination. What does a study of power help elucidate that a study of these related concepts would overlook? If the answer is that the book is teleologically oriented toward Foucault, then this should have been explained at the outset, along with the limits of this approach.

Second, what is the role of political participation *beyond consent* (or its absence) in the exercise of power in democracies? This question arises because of Hindess's intermittent invocations of Arendt's views, which are used vaguely to suggest the importance of consent for power (p. 11)—a most unsatisfying rendering of her writings on this subject, especially in *The Human Condition* (1958). Indeed, the practice of consent as it pertains to voting in a representative government is antithetical to participation, as Arendt argues in *On Revolution* (1963). Even when government is consensual (which is how Hindess construes Arendt's definition of power), norms of participation in modern states are overridden by those of interests. For Arendt, it is this slide of politics into interests that deforms the public sphere and deprives anyone of the possibility of exercising power. Especially because Hindess seems, in his conclusion, interested in a more fluid understanding of power, it seems odd that he does not detail the ways that aggregated individual consent—reflecting either false consciousness or true needs—may be at odds with political participation.

A related topic to power is that of civil disobedience, to which Hindess devotes no attention. Civil disobedience, with its appeals to a different notion of the public good than that inherent in any practice of positive law, complicates the conceptual terrain in ways that Hindess's definitions do not allow us to grasp.

The main quibble I have with Hindess's interpretations is that he ascribes to Foucault the notion that people are free and various regulatory regimes intervene to manage us, in ways that vary depending on the time and place, to wit: "Foucault conceives of power in terms of attempts to influence the actions of those who are free" (p. 18). The problem with this characterization, as Hindess himself may be recognizing elsewhere (pp. 149 and 151–2), is that Foucault deliberately sets out to wrench from us the notion that there is either a state of nature or a Utopia in which power does not exist (and freedom does). That is why in the first volume of *The History of Sexuality* (1980) Foucault refers to "liberation" in quotation marks, as what people may ever seek and cannot realize. If freedom is conceptualized as the absence of power, if power is exercised through discourse, and if all human societies have discourses, then it is misleading to write that "Foucault conceives of power in terms of a 'structure of actions' bearing on the actions of those who are free" (p. 97, citing Foucault's *Power/Knowledge,* 1980, p. 220), since no such people populate a Foucauldian worldview and because this reinforces the most basic kind of mistake undergraduates often make about Foucault—the confusion of what Foucault says about the "power/knowledge nexus" with what Marxists or critical theorists say about "ideology." (In brief, the former is an epistemic axis or set of taxonomic principles, while the latter is composed of falsehoods that serve the ruling class.)

Hindess concludes by calling into question the dominant approaches to investigating "fictional communities," which render them as functions of "political communities." Though the final sentences on the topic are not all that revealing as to what he has in mind, I take Hindess to be proposing a study of power along the lines of those engaged in the field of "cultural studies," namely, that it is the decentralized, uninstitutionalized manifestation of difference and domination that occupies attention. "In effect," he writes, "this means finding a way to think about politics in the absence of its defining, constitutive fiction: something far easier to suggest that it will be to effect" (p. 158). But if this really is the proper goal for political theorists investigating power, then it seems that Hindess would want to open with this problem, so that Hobbes, Locke, Lukes, and Foucault would be the relevant foils for beginning and not ending his investigation.

The Struggle for Recognition: The Moral Grammar of Social Conflicts. By Axel Honneth. Translated by Joel Anderson. Cambridge, UK: Polity Press, 1995. 215p. $59.95.

Horst Mewes, *University of Colorado at Boulder*

Honneth's aim is an ambitious one: he wants to lay the "foundations for a normative social theory" (p. 1). Its basis, in his view, must be the "logic of social conflict." Theories of conflict, he argues, have indeed provided the core of modern political philosophy since Machiavelli and Hobbes, superseding the theories of community at the heart of ancient political thought. Honneth decidedly sides with the moderns.

However, in modern political thought the dominant paradigm of social conflict has been the Hobbesian notion of the struggle for self-preservation. Social conflict is viewed in terms of the "reproduction" of the material interests of groups, classes, social movements, etc. Although Honneth does not intend to replace these materialist, utilitarian models of social conflict, much less challenge their "socio-ontological premises," presumably found in Machiavelli and Hobbes, he wants to "extend" them with an "alternative paradigm" of social struggle (p. 165). It is by way of this extension that a normative dimension will be added to otherwise purely utilitarian explanations of modern social conflicts. This alternative paradigm is Honneth's concept of "struggle for recognition" (or *Anerkennung* in the original German).

The original source for Honneth's paradigm is some work by the early Hegel that the latter neglected to pursue to theoretical maturity in his better-known masterpieces. It is essentially a theory of *individual* development embedded in a social struggle for love, rights, and solidarity. Its intent is to accentuate the *social* context for what must always be a *struggle* for individual autonomy. The desire for recognition, following these three stages of individual development, provides the normative standard Honneth needs for his normative social theory. I must leave the details for the interested reader to peruse by him- or herself. Since Honneth aims toward a "post-metaphysical" theory, the "greatest challenge" of his argument is to render Hegel relevant for today's world (pp. 69–70). The challenge is met, Honneth thinks, by George Herbert Mead's "naturalistic transformation" of Hegel's theory. By utilizing and refining certain parallels between Mead's "unsystematic social psychology" and Hegel's phenomenological ontology, Honneth is persuaded that contemporary empirical testing of his thesis of "struggle for recognition" will prove fruitful for understanding modern social conflicts and, consequently, provide the fundamental normative social theory he seeks.

But Honneth's theory does not, in his mind, merely explain the motives for social rebellion to be found in the fight against the violation of deeply held expectations for social recognition (p. 163).

Provided the empirical testing of the theory of "recognition" proves successful, Hegel naturalized by Mead presents us above all with an "idea of post-traditional, democratic ethical life." Their ideal consists of "a society in which the universalistic achievements of equality and individualism would be so embedded in patterns of interaction that *all* subjects would be recognized as both autonomous and individuated, equal and particular persons" (p. 175, my emphasis). This democratic idealism based on Mead's naturalistic social psychology "paralleling" Hegel's metaphysical ontology, argues Honneth, also provides us with a "critical framework for interpreting the processes by which societies develop" (p. 171). The democratic ideal provides us with the standard by which modern societies' development toward autonomous individualism can be viewed "as a historical process of moral progress" and as an "overarching developmental process" viewed from the vantage point of an "idealized path" toward the "normative potential of modern law and esteem" (p. 170). Honneth seems to suggest that Hegel's original view of modern history as the history of the freedom of absolute spirit, naturalized by Mead's social psychology, will provide empirical history with the normative notion of an ideal of democracy, which in turn provides the process of modern history with the concomitant notion of an idealized path of development. Honneth is looking for an empirically testable democratic idealism. Indeed, at one point he suggests this normative notion of an "idealized path" of development not merely for modern Western history, but the "*species-historical* process of individualization!" (p. 93, my emphasis). His initial Hegelianism, however much naturalized by Mead and however much postmetaphysical, dominates Honneth's idealism throughout. Does Honneth really mean to argue that he can provide the foundations for a social theory containing an ideal norm of posttraditional democracy, which in turn presents us with an "idealized path" for the historical development of the human species toward individualism? At this point my liberal scepticism gets in the way of my comprehension.

In conclusion, and short of a pursuit of the many sophisticated and often fascinating trains of thought to be found in Honneth's valuable book, I want to explore what Honneth means by a society in which the most extreme forms of individualism would still be embedded in "the solidarity-generating force of collective identity-formation" (p. 179). I should stress the fact, if it isn't clear by now, that Honneth's theory of social recognition does not provide for the contemporary politics of "group identity." The ideal of (Western? species?) historical development is individual autonomy within collective identity: "culturally typified status groups," along with, I presume, their contemporary forms of imitation, have been "devalued" along with traditional ethical life (p. 124). In the typical modern society, both reality and its ideal consist of *individual* struggle for (and enjoyment of) recognition, with its concomitant dimensions of private love, public rights, and encompassing social solidarity. However, the typical goal of having one's unique self-realization validated and recognized by society is in constant tension with the fact that it is precisely the "general value-horizon" or the "overarching system of esteem" (p. 126) of society as a whole that makes any kind of recognition possible in the first place. The key to "societal solidarity" under these conditions of individualism, Honneth argues, is a societal value-horizon with an essential "openness of interpretation" prohibiting "collective denigration" of individuals and allowing instead "individual competition" for social esteem. Individual recognition and esteem is thus assumed to still be based on the "contribution" of individuals to society, but contributions to the whole allowing for individualized renditions and enactments of what constitutes the good of the whole. Ideally, then, the solidarity of such an ideal democratic society would consist of the total absence of any form of disrespect. This absence alone, Honneth assumes, will still allow for individual competition for social esteem with unequal results (p. 129). We cannot, in this individualized ideal society, "esteem each other to the same degree." But so long as everyone is free from disrespect, free from collective denigration, free from "pain," such differences in social recognition and esteem will, Honneth implies, be meaningless (p. 130). But will not the unequal awarding of social esteem be tolerable to all only if society is always perfectly just in its awards, and, in addition, the absence of collective denigration has rendered all individuals just as well? Can a theory provide such prerequisites?

Obviously, today's fiercely competitive individualistic societies, marked by the pervasive dominance of individual status-seeking, fame, esteem, and celebrity recognition is a great distance from the ideal. And while it is one thing to empirically show the role of the struggle for recognition in past social and class conflicts, it is quite another to empirically show the possibility of Honneth's theory of combining the circle of modern individualism with the square of solidarity, to mix some metaphors. And indeed, Honneth at the end of his argument leaves this task, not to theory, but to "the future of social struggles" (p. 179). The future still awaits the emergence of a social "life-goal" that will harmonize competitive individualism with social solidarity. Honneth implies that modern societies still need democratic idealism to guide their social struggles. But his theoretical ideal still awaits its validation. Honneth's ideal theory is thus an appeal to the practical hopes contained in the future, from the vantage point of a present not unequivocally recognizable, I think, as the result of an "idealized path" of historical development.

Rights across Borders: Immigration and the Decline of Citizenship. By David Jacobson. Baltimore: Johns Hopkins University Press, 1996. 181p. $33.50.

On Nationality. By David Miller. Oxford: Oxford University Press, 1995. 210p. $24.95.

Jeff Spinner-Halev, *University of Nebraska—Lincoln*

While in some parts of the world states are inching toward integration with open borders and trade, elsewhere nationalism is showing its strength anew. David Miller declares that nationality has come to "dominate politics in the last decade of the twentieth century" (p. 1), while David Jacobson argues that the impact of immigration has made any "notion of territorially based self-determination patently impossible" (p. 133). These two contradictory claims initially seem to be reconcilable by discussing different parts of the world, but in fact both center their claims around the "Euro-Atlantic community." Miller wants to defend the idea of national identity in the West and bolster it in the face of its decline (a decline he alternatively denies and bemoans), while Jacobson shows that national identity and citizenship is becoming increasingly thin in the West.

Miller claims that nationality is a legitimate part of most people's identity. A few people may not feel the force of national identity, but most people will, including many who want to deny its strength, particularly in times of crisis. While nationality is only one aspect of most people's identity, Miller thinks that national identity is important because of the obligations it generates. He maintains that people who share a national identity are more likely to feel a greater obligation to help one another than people who lack this shared identity. Furthermore, they are more likely to trust and cooperate with one another. This increases the chance of a national state being just; it means, for example, that a nation-state will be more likely to have a fair welfare system than other states.

Miller also favors national determination because the nation-state can best defend a national culture; and a national culture is important to people's personal identity. Yet he sensibly qualifies his defense of national self-determination so that secession is justified only when the established state "houses two or more groups with distinct and irreconcilable national identities" (p. 113).

Miller, however, wants every national identity to be open to newcomers. Immigrants are not a threat to national identity since identity is always in flux, and national identity is molded by the different subcultures that exist within it. As immigrants adapt when they go to a new country, so the host country will change as it accepts these new immigrants. Miller points to the United States as a model here, since he thinks it shows how ethnic groups can retain their own identity while also taking on an American identity. The identity of white ethnic groups in the United States, however, has faded considerably. This melding together to form a national identity is a process Miller recommends to others; but here too the American lessons are rather limited, since American ethnic groups do not lay claim to land. It's the combination of identity and land claims that make national identity in other parts of the world rather bloody.

Miller argues that nations should not be attached to any group-specific cultural values. There must be a national language in a country, so complete cultural neutrality is not possible, but in many ways "national symbols and institutions can be detached from group-specific norms" (p. 137). This is an odd argument since the nation is, of course, a specific group. It would make more sense for a state to detach itself from specific group norms. This is one of many places where Miller elides the distinction between state and nation. This confusion appears when he says that part of what constitutes a nation is an "active identity"—through their proxies, like statesmen, soldiers, and sportsmen, nations act upon their decisions. It is states, however, that have soldiers, send athletes to the Olympics, and ambassadors to other states. When Miller argues that national cultures have a public dimension that include the architecture of public buildings, the content of education, and the character of television and film, he once again is really discussing the sorts of things that states control.

A culturally neutral state open to newcomers sounds very much like a neutral, liberal state, not like a state that has any kind of deep national identity. Given the openness of national identity that Miller prudently wants, it seems that two national identities can never be so irreconcilable that the secession he defends earlier in his book is warranted. It is hard not to get the impression that the nationality Miller defends is a thin Western variety, where the boundaries of state and nation already more or less match. Despite his protestations to the contrary, Miller's argument does little to shed any light on the national conflicts in Eastern Europe.

Miller's argument does show that an open democratic state's education, architecture, laws, and political institutions will influence the character of its people, but that all these things will change in response to immigration and other events. Unfortunately, Miller does not make the argument that a state's boundaries will shape national identity as explicit as he should have. This point is particularly important because of the way immigration is changing national and state identity.

Few people discussing national and cultural identity or citizenship have looked at the legal ramifications of immigration. David Jacobson fills this gap in his important book. Just as theorists have rediscovered the citizen, Jacobson shows how immigration challenges not only national identity in Western Europe and to a lesser degree in the United States, but also the very idea of citizenship. Immigration challenges citizenship because many immigrants have an unclear status. The guest workers in the Western European countries (Jacobson mostly discusses France and Germany) did not leave as they were supposed to and instead brought their families up to join them. But few of these workers have actually received citizenship in the countries where they work. Whereas in France the workers' offspring have typically received citizenship, in Germany they rarely do.

Immigration has created an odd group of people in some European countries, a group constituting not citizens, visitors, or guests. Since the 1980s these migrants have increasingly received the same civil and political rights that citizens have. While migrants cannot vote in most elections (though they can vote in local elections in some countries), this is the last major distinction between citizens and noncitizens. Because this distinction is not important to many (after all, how many citizens vote?), Jacobson argues that immigrants lack the incentive to become citizens. The key issue here, however, is not what happens with the immigrants, but what happens with their children. Overlooking their children's aspiration to become citizens and part of the national culture enables Jacobson to exaggerate the extent that immigrants see their home as somewhere else.

In his insightful argument that those interested in national identity, citizenship, and cultural diversity should note, Jacobson argues that because migrants have an uncertain legal status, courts in many European countries have increasingly relied on international or European legal codes to arbitrate disputes involving migrants. Migrants themselves appeal to international law when they go to a German or French court. Courts in different European states then use international law in cases involving citizens as well as migrants. Furthermore, European countries are increasingly subjecting themselves to the law espoused by European courts, which they must do to be part of important European organizations. International law, however, speaks not the language of citizenship, but that of human rights and universal "personhood." Not only does citizenship get eroded as the laws of different European countries increasingly come from international legal codes, but also differences between states disintegrate. If this process continues, the legal differences between European countries and the idea of national citizenship will fade.

The decline of citizenship and distinctive national identities does not mean the decline of the state. Rather, the state becomes quite important as the bureaucratic agency that enforces international law. For example, the Organization for Security and Cooperation in Europe (OSCE) declares that member states should seek not only legal but also actual gender equality. This means that member states should "establish or strengthen national machinery . . . for the advancement of women" (p. 112).

Miller is aware that Western Europe is moving toward the integration that Jacobson describes, but he insists that national identity still thrives in Western Europe. In the case of Britain, though "there is a strong sense that the British do have a separate identity . . . it is far from clear what this separate identity is supposed to consist in" (p. 172). It may very well be that if the separate national identities in Britain and Western Europe are shallow, the importance of these identities will fade over time. Miller seems to admit this possibility, since he suggests ways to shore up national identity. Having public debates about the character of national identity, instituting a civic education sensitive to cultural pluralism, and establishing a written constitution (in Britain) hardly seem enough, however, to do the job.

Miller wants to strengthen national identity because the world culture that would take its place if nationality declines is banal. While the elite would have the money to enjoy the best that a world culture has to offer, the masses would be subject to a culture "exemplified by Disney, McDonalds, and Australian soap operas." Furthermore, the masses would find themselves in an increasingly precarious economic position, subject to the vagaries of a global economy and without the national solidarity necessary to induce others to help them out in times of need.

These worries are important ones, and Miller's defense of national identity provides one way to think about how to deflect them. His sensitivity to the way national identity and inclusiveness might cohere in a world where boundaries are increasingly porous is to be applauded. But Miller's argument does little to rebut Jacobson's contention that these permeable boundaries threaten not only national identity but also citizenship. Jacobson's look at immigration and the legal responses to it should be a sober warning that the recent resurrection of citizenship may very well be exaggerated.

Hayek's Social and Political Thought. By Roland Kley. New York: Oxford University Press, 1994. 248p. $55.00.

Gordon Tullock, *University of Arizona*

This book can be best summarized by its last three sentences: "Markets must, as [Hayek] rightly insists, play an important role. How far they should extend, and how far they should be constrained, and what ways supplemented and in what kind of political framework they should be embedded cannot be decided on grounds of feasibility alone. Answering these questions requires genuine moral reflection, and falls within the province of normative political philosophy" (pp. 228–9).

Hayek was a truly great economist, and like many other great economists, physicists, biologists, etc. he tended to feel that his disciplinary specialization gave him a considerable head start in other areas. In his case he wrote a great deal of what I would regard as "normative political philosophy." He certainly reflected on it a great deal.

This book is by a political philosopher who is essentially attacking Hayek in that area. It should be said that from time to time he criticizes Hayek's economics. Normally, when he does this he is wrong, but this is not an important part of the book.

One of the reasons why he is wrong about his criticism of Hayek's economics is that Hayek's work is spread out over a very long time. Kley sometimes thinks that Hayek's comments that were actually aimed at the then-popular Keynesian policies refer to the present problems when Keynes is long dead. (See p. 43 for a particularly good example.) But that is not the main theme of Kley's critique.

Basically, he goes through Hayek's very extensive writings on politics and morals. He ignores his work on psychology for some reason. Kley's criticism frequently involves first systematizing Hayek to a higher level than he himself used, and then pointing out that the particular systematization does not work. He says repeatedly, for example, that Hayek's position requires a list of, say, five preconditions, and then says that this list is wrong. In general, his statement that the list contains errors is correct, and his statement that Hayek requires the listed preconditions is more or less accurate. It does tend to make Hayek a much more systematic thinker in this area than he was.

I am not saying that Hayek thought of himself as a good economist but only a moderately good political scientist or philosopher, but that is the fact. Furthermore, his work in political science and philosophy is dominated by his firm economic views. Kley does not share these views, although he is not firmly on the other side either. (No one is since the fall of communism.) He mainly attacks the moral philosophy or political science on its own grounds.

To repeat what I said above, Hayek was only a moderately good political scientist or philosopher. (I should say that Hayek's view of my work was much the same: He thought I was a good economist but didn't like my political science.) Kley finds various things wrong with Hayek's work in this field. This is a case of the blind criticizing the blind. We still do not know much about normative philosophy. Hayek and Kley talk about the need for reflection, and this is certainly needed, but the problem is that different people who have reflected get different results. Hayek certainly engaged in as least as much reflection in these areas as Kley. The fact that Kley and he have different ideas as the outcome of such reflection is exemplary of the difficulties in these fields.

Consider another man who has devoted most of his life teaching law at the equivalent of university level, and certainly taught it with a highly ethical overtones. He eventually reached a place where his moral philosophy could be applied. So far as I know, Kley would not regard the long period of careful reflection and thought by the Imam Khomeini, as indicating that he had reached the right conclusions. There is no reason to believe that Hayek—or for that matter, Kley—has.

The Pleasures of Virtue: Political Thought in the Novels of Jane Austen. By Anne Crippen Ruderman. Lanham, MD: Rowman and Littlefield, 1995. 202p. $57.50 cloth, $21.95 paper.

Ethan Fishman, *University of South Alabama*

This is an unusual book. It takes the texts of novels seriously, even suggesting that they might offer valuable insights into classic political and ethical issues. It offers an original explication of Jane Austen's prose. It posits the existence of universal ideals. And it demonstrates the merits of studying literature and politics.

The discipline of literary criticism today seems to be dominated by postmodernists who deemphasize the text and disregard the authority of the writer. Ruderman takes the contrary position that Austen consciously develops certain themes in her fiction about the individual, society, and transcendent moral values. In the past scholars have been divided as to whether Austen's novels defend individualism or celebrate social conventions. Ruderman argues that Austen's novels actually present both views simultaneously.

According to Ruderman, it doesn't really matter to Austen whether we choose to find fulfillment through self-expression or in conformity with long-standing rules of propriety—as long as our fulfillment derives ultimately from service to others. Ruderman illustrates her argument by referring to the portrayal of marriage in Austen's novels. Against those feminists who consider marriage to be an institution invented by men to oppress women, Ruderman's Austen looks upon it as a natural relationship between the sexes.

Austen's successful married couples experience true love through shared values, companionship, and stimulating discussion. What they discover is that they can develop themselves as individuals more effectively when they strive to realize their marriage vows. Austen even intimates that sex becomes more pleasurable when it is performed by husbands and wives who display profound concern for each other. Ruderman does not ascribe to Austen the view that human beings are unable to lead fulfilling lives outside of marriage; Austen herself was a spinster. Her position is only that the institution provides the social structure and emotional support most people need to sustain a loving relationship.

The discipline of political science today appears to be dominated by relativists who deny the existence of universal ideals and oppose the concept of intrinsic worth. For Ruderman, however, one of the central themes of Austen's novels is that virtue, defined as loving your neighbor as yourself, is its own reward. Those protagonists choosing to live virtuously, Ruderman argues, are portrayed by Austen as having reached their potential as human beings, who alone among animals possess the ability to make rational and moral decisions. When these same characters discover that their choices lead to personal happiness and pleasure, Ruderman maintains, it only serves to confirm the value of virtuous behavior.

Although Ruderman does not consider Austen to be a political philosopher or even a student of political philosophy, she does discern a close connection between Aristotle and the ideas expressed in Austen's novels. Ruderman's thesis, in essence, is that knowledge of Aristotle's theory of moderation can help readers interpret Austen's prose. I would add a corollary to her thesis: Those who read Austen can gain fresh insights into Aristotle's thought. I also wonder why Ruderman neglects to make the connection between Austen's definition of virtue and the biblical Golden Rule.

Great authors of literature and great political philosophers share a common interest in the perennial issues facing humankind. While the philosophers offer theories to explain where and how they stand on the perennial issues, the authors demonstrate the practical effects of applying these theories to our daily lives. In the final analysis, it is this mutuality of interests and diversity of approaches that make the subject of literature and politics worth studying and Ruderman's book worthy of both our attention and praise.

Paradoxes of Modernity: Culture and Conduct in the Theory of Max Weber. By Wolfgang Schluchter. Translated by Neil Solomon. Stanford: Stanford University Press, 1996. 389p. $49.50.

Nancy L. Schwartz, *Wesleyan University*

Wolfgang Schluchter, a leading scholar of Max Weber, here contributes a work that has not previously been published in German and, in a key chapter, completes an important and missing link in his analysis: the study of Islam. Overall, the book argues that by analyzing Weber's sociology of religion alongside certain of his political writings, we can discern a typology of ethics that helps us understand the meaning and causality of Western economic development. This argument is continuous with Schluchter's earlier work and is here taken to broader arenas and deeper levels, and is sustained throughout. Schluchter's claim of unity is controversial since some commentators have seen a split or disjunction between Weber's normative writings and his comparative studies; Schluchter overcomes this split.

The four chapters in the book read as four distinct essays and, indeed, chapter 4 refers to itself as "this essay." The chapters are consistent with each other as the product of a rich and systematic mind, but they are written in different styles with somewhat different aims and with over one hundred pages of notes and references. One has to step back and do some acts of intellectual construction and streamlining to connect them. Perhaps the author intended for us to do this.

Weber is shown to have an "ethically grounded realpolitik" position that is not mere power politics nor relativism. The ethical ground comes from what Schluchter—in fascinating dialogue with Immanuel Kant, Jürgen Habermas, Albrecht Wellmer, and others—calls Weber's "criticist" rather than "cognitivist" position. Weber generated two "ethics of reflexive principle" in his famous ethics of conviction and responsibility, and these are distinguished in terms of rules, motivation, and sanctions. The ethic of responsibility, Weber's ultimate position, is a principle of critical examination rather than a principle of justification.

Weber, the argument implies, applies this ethic both to his assessment of political action and to his scholarly work. It is not always clear in this book whether the typology of ethics is applied to the sociology of religion or generated from it, or both. Nor is it apparent whether and how Weber's own ethic of responsibility plays a role in his analysis of world religions. This brings us to the touchy question of Orientalism and the "non-West," issues raised in recent years by writers such as Edward Said in *Orientalism* (1978, p. 259). Is Weber guilty of objectifying non-Western religions and culture to an "other" in their non-modernity and non-Western qualities? Schluchter would respond no, or yes only insofar as Weber also did this with Lutheranism, Catholicism, the Eastern or Orthodox Church, and Judaism—religions within the "Western" heritage whose rationalities contrasted with the Calvinist Protestantism Weber sees as key to modern "inner-worldly asceticism."

The chapter on Weber's analysis of Islam, or "hindrances to modernity," is the central and longest chapter in the book. It is also the most powerful and interesting. The writing moves along and Schluchter attempts an original feat: to reconstruct what Weber would have written about Islam had he lived to complete that part of his work. Marshall Hodgson, in *The Venture of Islam: Conscience and History in a World Civilization* (1974, vol. 1, p. 133, n14), wrote, "It is unfortunate that [Weber] said so little about Islam." Schluchter has now tried to fill this gap.

Islam has a conception of a transcendent God similar to Calvinism's and an idea of predetermination, but the latter notion is not linked to an idea of proof of salvation as in the Calvinist idea of predestination. Their practical ethics also differ: Islam promotes self-sacrifice in extraordinary situations and this-worldly utilitarianism in everyday life; Calvinism's asceticism of everyday life has a certain heroism and sees this activity as a sign, if not proof, of election to the afterlife. Islam came into being as a religion of the ruling stratum, rather than of the bourgeoisie as in Calvinism.

Schluchter then moves into his original reconstruction: using fragments from Weber's writings on political domination, on the city, and on the sociology of law and the sociology of religion, he proposes some Weberian comparative generalizations about the mentalities and institutions of Islam. For example, in Islamic states feudalism is said to have been less decentralized than in Western states; central territorial rule extended more directly into cities; law remains sacred and the profane administration of justice becomes "substantively irrational" (a term from a Weberian typology that Schluchter wisely decides not to use here). In contrast, the papal, feudal, and urban revolutions of the eleventh to thirteenth centuries in Western Europe established crucial political preconditions—normative law, the corporation, citizenship—for Weber's modernity. Schluchter explores the paradoxes in some of these developments, such as the paradox of a highly unified Church culture that also allowed a certain pluralism. Schluchter also discusses several paradoxes in the "non-modern" institutional and ideational life of Islam.

He does not, however, directly discuss the paradoxes of modernity, which is this book's title. An earlier essay of his, "The Paradox of Rationalization: On the Relation of Ethics and World" (in Roth and Schluchter's 1979 *Max Weber's Vision of History*) is not footnoted. The problem of meaninglessness in a world where we are free to choose meaning is alluded to in the first and last chapters here. It could be important for Schluchter to discuss these paradoxes of modernity more directly, especially since postmodernity is celebrating what Weber surely saw as tragic. Schluchter would be equipped to do this, for he shares Weber's "aristocracy of spirit."

Mass Enlightenment: Critical Studies in Rousseau and Diderot. By Julia Simon. Albany: State University of New York Press, 1995. 236p. $59.50 cloth, $19.95 paper.

Rousseau and Liberty. Edited by Robert Wokler. Manchester and New York: Manchester University Press, 1995. 299p. $69.95 cloth, $24.95 paper.

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Of all the great political theorists, Rousseau is perhaps, with Nietzsche and Plato, the most protean. The range of interpretations lends itself to every political position on the spectrum and, increasingly, to every interpretative approach. Such motility is for some a sign of confusion or diffusion in the original texts; it is, in my book, a sign of great spiritedness. *Plus c'est la même chose, plus ça change.*

The two books under review here evince some of the richness that now surrounds Rousseau. The book edited by Wokler contains thirteen papers from a conference on "Rousseau and the Cause of Liberty." At the risk of overemphasizing common traits, we can characterize them, from a critical point of view, as traditional: Lévi-Strauss is the only major contemporary thinker to appear in the index. Julia Simon's book, in contrast, begins with a citation from Lyotard and focuses on the appearance of "mass culture" in the works of Rousseau and Diderot. She understands mass culture explicitly in the way that Adorno and Horkheimer do. Her index includes Foucault, Habermas, and Derrida, and whereas that in the Wokler volume goes directly from "Bosanquet" to "botany," all of Simon's entries under the letter "b" start with "bourgeois," a choice she spends a page justifying.

What are the strengths and weaknesses of each book? In the Wokler volume, we find first in a fine essay by Patrick Riley a Rousseau who has no universal morality and for whom the general is always "somewhat *particulière*" (p. 20). Then, in an essay comparing Rousseau to Smith and Hume, Norman Barry finds him also to be a potentially dangerous "communitarian in inspiration ... [endeavoring] to suppress certain preferences" (p. 50). A variant on this conclusion, but with a more positive valuation, appears in Christopher Kelly and Roger Master's excellent work on the reception to the *Discourse on the Origin of Inequality.* They show that Rousseau's concept of perfectibility allows him to "defend a particular sense of liberty and responsibility [and guides him] through a minefield of metaphysical complexity" (p. 65). Melvin Richter adds an interesting historical footnote to these questions in his analysis of the dialogic influence of Rousseau on Tocqueville.

Much of the same turns up in the next set of essays focusing on the *Social Contract.* Geraint Parry associates Rousseau with the target of Amy Gutmann's nasty (and unfair) gibe at communitarians who "want us to live in Salem but not to believe in witches" (p. 118). John Hope Mason finds Rousseau less fully in the communitarian camp but "pulled ... in contrary directions, acutely aware of the individual self as a distinct entity, and at the same no less conscious of the sociological perspective." The resolution of this "difficulty" is deemed "impossible" (pp. 133–4). John Charvet continues the English tradition of reading Rousseau as Hobbes in French lace with an essay that insightfully links Rousseau with Hobbes's notion of "absolute sovereignty" (p. 145).

Felicity Baker, in what to me is the single best essay in the volume, discusses Rousseau's understanding of the death penalty and the question of human evil. (I might note that the citation of Lévi-Strauss occurs in her essay.) Her analysis is nuanced and complex, starting from the realization that "it is precisely where Rousseau identifies the most impersonal, modern form of social evil that he proposes the most archaic punishment" (p. 167). Almost alone in this volume, Baker realizes that reading Rousseau is not as straightforwardly easy as he might make it seem.

The last section is a grab bag of essays. Robert Wokler returns, with his deep knowledge of Rousseau, to the political significance of reverie for Rousseau's political thought. In an interesting essay, Ursula Vogel explores the complexity of Rousseau's understanding of gender: "While he did take sides with some of the most restrictive and inequitable traditions, he endowed them with new meanings by linking them to ideas and apprehensions" (p. 228). The late Maurice Cranston provides a careful understanding of the different forms of liberty in Rousseau. Lester Crocker returns to themes he has previously pursued with the argument that Rousseau "believed . . . in the possibility of conceiving, if not realizing, a perfect social order, a rational order, a true society engineered in any historical vacuum" (p. 263). Finally, Iain Hampsher-Monk also returns to the theme of Rousseau and totalitarianism and suggests that Rousseau's work offers us a more fertile debate than that between "negative liberty-based liberalism . . . and a conception of positive liberty which sees the specter of the Gulag behind every attempt to specify it" (p. 283).

Space does not permit further exploration of the Wokler volume. Suffice it to note here that with the exception of Baker's essay the notion of *community* remains singularly unproblematized for all these authors, no matter whether they are favorable to or suspicious of it. It is as if community came easily to everyone's lips but that none had looked to their heart to see of what they (or Rousseau) spoke. What is it that Rousseau thinks we can have in common, that we can share, that is our portion?

As noted above, Julia Simon approaches Rousseau and Diderot with considerable explicit conceptual baggage, which she sets out and justifies very well in her introduction. How much difference does it make to our understanding of Rousseau and Diderot? At first cut, not very much. "Rousseau saw the corruption of humankind in the development of the arts and sciences, while Diderot capitalized on enlightenment as editor of the *Encyclopédie*" (p. 18). In her first chapter on Rousseau (a reading of the *Discourse on the Origin of Inequality*) she concludes that Rousseau overemphasizes the individual and individual expression: "Rousseau's attempt to preserve individual autonomy marks the historical appearance of mass culture." However, she also says that "protecting the individual's freedom and autonomy in the face of the division of labor and commodity fetishism requires elaborate social engineering on Rousseau's part" (p. 43). For Simon, as for so many others, but now for critical theory reasons, totalitarianism lurks in Rousseau. In her second chapter—a reading of the major explicitly state-oriented texts—Rousseau is portrayed as using "state-controlled mass-culture in order to prevent the development of social disintegration," a policy that is no better than submitting to "mass culture determined by market forces" (p. 69). Her last chapter on Rousseau attacks Rousseau's autobiographical writings as an attempt at preserving the self from the public sphere, an attempt Simon holds to be impossible.

There is one new element here, or, more accurately, Simon's critical theory approach foregrounds an element that we can now see was implicit in earlier interpretations. Extending a course mapped out by Robert Darnton, she notes that *La nouvelle Héloise* was a best-seller in the modern sense of the term and that this in turn generated market strategies to produce new best-sellers. The "public" becomes a problem; what is more, Rousseau, she argues, was aware of this problem. "Rousseau was particularly sensitive to issues that concerned alienation, commodification and ultimately domination. His [works] testify to his concerns about rationality in the service of technological development and its potential to overrun the public sphere. Keenly aware of the problem of private interest, he forecasts the growth of mass markets" (p. 100). Yet *plus ça change, plus c'est la même chose.* The Rousseau that Simon gives us succumbs before the fact to the dialectic of enlightenment. For all of Simon's argument about Rousseau's apparent preference for isolation being a "hedge against technological domination" (p. 95), this is in the end a very traditional Rousseau, caught between his individualism and his supposed social engineering. For Simon, he never fully grasps the nature of the relationship between the private and public spheres.

The true progenitor of critical theory therefore turns out to be Diderot. For Simon, Diderot is a theorist of a dynamic materialism that understands, accepts, and can explain political change (in an analysis of *Le Rève d'Alembert* in chapter 4). In *Rameau's Nephew* (which Simon explicitly reads in the way Hegel does) Diderot provides a "cynical commentary on bourgeois society" (chap. 5). In the last chapter (with a certain amount of acknowledged help from Michael Fried's book on this topic) she provides a reading of Diderot's art criticism that shows how it anticipated Kant's understanding of aesthetic judgment. Here Diderot reveals "the fundamental interdependence between the bourgeois public and private spheres" (p. 173).

For all of this, we have not moved conceptually much beyond Marx's "On the Jewish Question." Over against the more traditional interpreters of Rousseau, Simon at least shows us that the apparent tensions in Rousseau's thought point to a resolution of which he may not have been aware. She assumes, as do most of the contributors to the Wokler volume, that these tensions are those (and are as) she and these contributors find them to be: so why did Rousseau think his work, as he repeatedly proclaimed, to be of a piece?

Moral Rights and Political Freedom. By Tara Smith. Lanham, MD: Rowman and Littlefield, 1995. 225p. $54.50 cloth, $21.95 paper.

Jack Donnelly, *University of Denver*

Moral Rights and Political Freedom is a book with a mission. "Individual rights today stand in grave danger" (p. 1), Tara Smith argues, due to an inflation in their number and a consequent devaluing of the currency of rights. "To stem the current inflation, we require an objective standard by which to assess the validity of alleged rights" (p. 5). Smith contends that a conceptual and linguistic analysis shows conclusively that "rights protect one thing: freedom of action" (p. 5). "Individuals do possess rights to life and property, but we do not possess rights to welfare" (p. 10). But both her defense of rights to freedom and her arguments against the existence of other moral rights are seriously flawed.

"1. Human life requires productive effort. 2. Productive effort requires reasoned action. 3. Reasoned action is individual and self-authored. 4. Reasoned action requires freedom. 5. Thus if we seek to live in a society in which individuals are to have a chance to maintain their lives, we must recognize individual rights to freedom" (p. 33). Deductive arguments to such substantive moral conclusions are notoriously controversial. In the area of moral rights, consider Gewirth's (*Reason and Morality*, 1978) much discussed argument to a somewhat broader conclusion. But whatever the attractions or problems of this particular argument, the entire effort is fatally flawed by shifting meanings of the key term, *life.*

"The need for rights arises from basic facts about the requirements of survival" (p. 32). "The first premise makes a straightforward factual claim. The survival of human beings is contingent on the fulfillment of certain needs.... If a person is to preserve her life, she must see to it that various needs are met" (p. 33). Smith thus explicitly defines *life* as the basis of rights in minimalist terms as survival. But in responding to objections—most notably the fact that only a few need act freely and reasonably for many to live (survive)—she completely, and with absolutely no justification, shifts meaning. "The life at issue is that of a human being doing well.... The end of rights is the good life" (p. 43).

Beyond the obvious problems raised by such inconsistency in a deductive argument, this causes problems for Smith's later case against economic and social rights. While her focus on production as the (sole) precondition for (mere) life is debatable, but plausible, any conception of the good life certainly cannot have production as its sole precondition. Therefore, this argument for rights to freedom, even if it were to go through, does not support a claim that the *only* moral rights are to freedom.

The second major move in her argument against moral rights other than life, liberty, and property is conceptual. "Rights must be absolute in order to perform their function" (p. 111). "Nonabsolute rights provide a person with no means of knowing whether she should abide by the relevant rights in a given case or look beyond rights to additional considerations" (p. 111). This is a patently false empirical claim. For example, very few legal rights are absolute, yet they usually provide considerable, and often entirely sufficient, guidance for both right-holders and duty-bearers. Smith confuses lack of certainty with lack of knowledge. But this argument is at worst weak, and thus controversial in ordinary ways.

Her final major move, however, again resorts to a shift in the meaning of a key term. Smith argues that the essence of freedom is the ability to govern one's own actions. This, she plausibly contends, can only be infringed through force. In defining *force*, Smith notes among the standard meanings "constraint or compulsion exerted upon a person." "The gist of these accounts is that force is the exertion of pressure on an object that alters its condition in some way" (p. 142), "against the recipient's will or without her consent" (p. 145).

But Smith then goes on to exclude nonphysical force by insisting that it "cannot control ... the very mechanism from which ... decision emanates" (p. 149). This shifts the focus of the argument, without justification, from the defined domain of constraint, coercion, and pressure that alters the object's condition "in any way" to the realm of control—which, of course, physical force often does not produce either!

"Only physical force can disrupt a person's ability to direct her own actions" (p. 123). This is obviously false. One need not be a bleeding heart liberal to suggest that severe and extended psychological abuse is at least as disruptive of one's control over one's actions as short and minor physical force (e.g., tickling or pinching). It is not even clear that physical force is the best way to subordinate another to one's will (p. 152). "Purely intellectual means cannot compel" (p. 148). Perhaps—although this is by no means clear. But force has been defined to include pressure and duress short of compulsion, and nonphysical pressure or duress is a regular occurrence.

Although its main argument fails rather dismally, some parts of Smith's book are of interest. For example, chapter 3 presents a direct defense of the "egoism" of rights. This will be particularly provocative for defenders of human rights with less libertarian inclinations. Chapter 4 argues plausibly, although not particularly originally, against consequentialist and deontological theories of rights, and briefly, but intriguingly, for a teleological justification. The discussion of the right to property (pp. 189–94), although short, is strong and unideological. The central arguments of the book, however, are so seriously and obviously flawed that those who disagree with Smith are unlikely to feel challenged and those who agree with her are unlikely to find much useful support.

Herodotus and the Origins of the Political Community: Arion's Leap. By Norma Thompson. New Haven and London: Yale University Press, 1996. 193p. $25.00.

Clifford Orwin, *University of Toronto*

See that woman with the bronze-tipped ram battering at the gates of the pantheon of political theory? She's not trying to demolish the edifice but to compel the admission of one unjustly excluded. To hear Norma Thompson tell it, the "canon" has discriminated even against a dead white Hellenic male.

Thompson's villain is Aristotle, whom she blames for the low repute in which Herodotus came to be held as a thinker. "Aristotle scorned Herodotus' 'ancient' way of thought as chaotically inclusive of virtually all dimensions of human life" (p. 7). It is precisely this greater inclusiveness that Thompson sets out to rehabilitate. She argues that in this postmodern age, when old models of rationalism are under fire, it is time to reconsider a still older one. "Herodotus' work offers a new foundation in support of Aristotle's perception that man is the *political* animal. At the same time it provides an alternative to Aristotelian-type theory about that fundamental reality" (p. ix).

What is this Herodotean alternative? It is to understand each human community on the basis of the narratives that lie at its core. Herodotus is so puzzling a writer for the same reason that he is so fascinating: he proceeds by means of storytelling. The basic question of Herodotean interpretation has always been what to make of these stores and Herodotus's relation to them. This is where Thompson shines.

As the protagonists in Thucydides are not individuals but regimes (Athens and Sparta), so in Herodotus they are "cultures"—notably the Persian, Egyptian, Greek, and Scythian. This insight is not new with Thompson, but her development of it is thoroughgoing and radical. According to her, every tale in Herodotus is significant primarily for what it reveals about the people whose tale it is—ultimately, for what it reveals about their respective self-understandings. For Thompson's (somewhat Heideggerian?) Herodotus, the political community is constituted by the tales that peoples tell. Not just from what a people does, but from what it says about what it does, from the whole narrative background and justification of its practices, does the character of its polity emerge for us. Indeed, the deeds prove to follow above all from the different tales in light of which human beings act; we are both enabled and limited by the community of taletelling to which we belong.

As noted, Thompson carries this approach very far. Consider the "debate of the regimes" (Herodotus 3.80–3), in which three Persian grandees, having deposed an impostor from the Persian throne, debate whether to institute a democracy, an aristocracy, or a monarchy in his place. Conventional wisdom holds that this episode is utterly implausible as a description of an event alleged to have occurred among the Persians and is important rather as a reflection of Greek opinions current in Herodotus's day. Thompson, however, argues that this account is not extraneous to Herodotus' Persian narrative but integral to it: it is significant not as a general statement that transcends its context but for the light that it sheds on the Persians. The tale of the debate is not rendered implausible by the fact that in the end the Persians merely reestablish the despotism under which they had lived before; rather, their failure to seize the opportunity to do otherwise displays that peculiar blend of rationalism and defeatism characteristic of the Persians throughout.

Proceeding in this way Thompson reveals how rich and varied and yet consistent are Herodotus' portrayals of the various "cultures." Her treatment of the Persians is particularly brilliant, but she is fine on the Greeks and Egyptians as well. While denying that Herodotus is a triumphalist booster of Greekness, she enters the provocative claim that his Greeks are indeed superior to the extent that they mirror (however imperfectly) Herodotus' own receptiveness to the wisdom of non-Greeks. While the narrative worlds of both Persia and Egypt are closed, that of the Greeks is (somewhat) open. While the gods of the non-Greeks foreclose debate, those of the Greeks (who express themselves only through ambiguous oracles) invite it.

I have offered a simplified account of Thompson's thesis; I can't begin to do justice to the richness of her interpretations of particular tales in the work and their interactions. She is a subtle reader and a lucid and graceful writer. She excels at bringing to bear on Herodotus (and at bringing Herodotus to bear on) current debates about historical objectivity and the historian's role. Brief thematic treatments of Martin Bernal, François Hartog, and Edward Said demonstrate not only the inadequacy of their readings of Herodotus but the superiority of his approach to the problems of "cross-cultural" history.

Nobody and no work is perfect. The political scientist might wish for a more thorough treatment than Thompson provides of such issues as the regime and justice (for such is to be found in Herodotus). No note is paid to such resoundingly cross-cultural declarations as, for example, that the proto-Rawlsian marriage auction of the Babylonians is the wisest of their laws (1.196) or that their law of sacred prostitution is the foulest (1.199). Similarly one might question Thompson's inattention to issues of nature and necessity, as if Herodotus understood man as no more (or less) than the being who creates himself communally through his taletelling. "The task then is to fulfill the human vocation, to create history through art and to form community through that perception. . . . What we make of [history] will shape our common destiny" (p. 167). Such may be Thompson's modern/postmodern view, but there is reason to doubt that Herodotus was quite so hubristic.

Real Freedom for All: What (If Anything) Can Justify Capitalism? By Phillipe Van Parijs, New York: Oxford University Press, 1995. 330p. $35.00.

Howard J. Sherman, *University of California, Riverside*

Van Parijs argues that capitalism has an unacceptable level of inequality and that freedom is a high priority. He recognizes and states in great detail the conservative argument that freedom necessitates inequality. People must be allowed to do as they wish in the economy without government interference and this is the only road to freedom; intervention for equality is the road to serfdom.

The most important contribution of the book is a rigorous new argument to refute the conservatives and prove that equality is not only compatible with freedom but also is necessary for a real freedom. For example, in our present society all people have the formal freedom to say what they want in any forum that they can afford. How many people, however, can afford a half-hour of prime time on CBS? For most of us, including well-qualified presidential candidates, that is a completely impossible dream. We have the negative freedom to say what we want without government interference, but we do not have the positive freedom to say it in the media. We can say what we want only on the street corner, and this no longer counts for much. Similarly, we have the right to buy health care or a new house, but many of us do not have enough money to do so. Real freedom means having both the negative formal freedoms and the actual positive freedoms, so it implies a large measure of equality. Van Parijs expresses this argument in different terms and in an innovative manner, elegantly with axioms and rigorous deduction. This is an impressive and important achievement.

On this basis, the author argues there should be a basic guaranteed monthly income for all, regardless of performance. Only a guaranteed basic income allows individuals to have considerable economic equality and to participate more equally in politics. He finds that freedom and justice both require a basic guaranteed income at the highest level that society can afford because it is the basis of all freedom.

Conservatives will resist his proposal, and, while recognizing its value, so will some leftists, who are likely to have three problems with the book. First, he uses the methodology of individualism as developed by both the rational choice Marxists and the neoclassical economists. This approach begins with artificial assumptions about individual preferences, such as that all individuals choose rationally among alternatives and that society then arrives at an optimal equilibrium outcome. Many radical social scientists disagree with this approach on the grounds that this explanation cannot be reduced to individual psychology, that individuals must be explained as part of the social matrix, and that the processes of (limited) choice in democratic capitalism do not lead to automatic equilibrium in economics or politics. On the contrary, most radicals argue the need to begin with institutions and relations (including class, race, and gender) in a society that has conflicts and that continually evolves.

Second, Van Parijs envisions paying the basic monthly income in money because this allows more individual freedom of choice. Many radicals have argued that it is better to provide free goods and services (such as education or health care) rather than money. The argument is that the expansion of a free sector undermines not only the interests of the elite but attitudes of commercialism as well. Any such change should be gradual, so that the expansion of the free sector might equal the growth rate, thus preventing inflationary pressures in the rest of the economy and making the change in incentives much less noticeable. In the United States probably the most urgent areas for free goods and services would be health care, education, and public housing.

Third, many radicals would question some of Van Parijs's views on whether capitalism or socialism would be more help to his goal of real freedom. Van Parijs does find inequality under capitalism "unacceptable" and assumes that socialism would lend support to more equality. He argues, however, against what he calls the Lockean, Lutheran, or Marxian forms of exploitation because his political economy is based on psychological axioms and an assumed freedom of individual choice, not on class relations or on specific institutions.

The author assumes that it is obvious from recent history that capitalism is far more efficient than socialism. Since he defines *socialism* as nothing but public ownership, so the old Soviet Union and all of the Communist countries were "socialist." Even if socialism was less efficient than capitalism in these countries, is it obvious that the inefficiency was due to public ownership? The Soviet Union used central planning rather than market socialism; would a market socialism be less efficient than market capitalism? Moreover, the Soviet Union was not only excessively centrally planned, it was also undemocratic. Surely, the existence of a dictatorship might have something to do with its inefficiency. In real Soviet history, one cannot disentangle the effect on economic performance of Soviet central planning from that of Soviet dictatorship, so one cannot condemn a system of democratic central planning on the basis of the Soviet experience. Even if the free market is more efficient than the planned, why should market capitalism be any more efficient than market socialism?

Nor does Van Parijs give any weight to the cyclical depressions of capitalism that cause so much human misery. He assumes that government fine-tuning can prevent involuntary unemployment. Yet there is no evidence in the real world that such fine-tuning has succeeded in preventing massive involuntary unemployment—as well as a long-run decline of real wages.

This is a book whose excellent analytic presentation will be much admired by all followers of rational choice theory. Those on the Left who share the author's ethical values will find the central argument—that real freedom entails equality—to be definitive and persuasive. Conservatives will detest the book's insistence on the need for a basic guaranteed income. Many radicals will reject the abstract argument about "capitalism" versus "socialism" because it is based on ahistorical individualism and abstract deduction. Nevertheless, the analytic power of its central argument will make the book required reading for most political theorists and their graduate students.

Arendt and Heidegger: The Fate of the Political. By Dana R. Villa. Princeton: Princeton University Press, 1996. 329p. $55.00 cloth, $19.95 paper.

Fred Dallmayr, *University of Notre Dame*

Finally a book about Arendt and Heidegger that one can read with intellectual benefit—and without embarrassment! Recent years have seen the emergence of a genre of writing on this topic that, in its taste for gossiping and lurid depictions of intimate details, approximates the level of the tabloid press; typically, in these depictions, the Heidegger-Arendt relation is portrayed as the encounter of an aging rake and a naively trusting *ingénue.* Villa's book is far removed from this genre, and properly so. If there is a point to Arendt's distinction between public and private domains and her resistance to forms of indiscriminate publicity, this is surely a place to respect her teaching.

Attentive to this point, Villa presents Arendt and Heidegger as thinkers or writers of the first order whose intellectual contributions must be assessed in their integrity (though, of course, not uncritically). Basically, his approach is to compare and contrast Heidegger's and Arendt's respective works with the aim of illuminating salient areas of contact and divergence. In his own words (p. xi), his book uses Heidegger to highlight "relatively reflected dimensions of Arendt's work," while simultaneously using Arendt to criticize Heidegger's "philosophical politics." This is a plausible approach and one that proves on the whole quite fruitful. As Villa notes correctly, Arendt has often been appropriated by theorists of our time—from liberals and communitarians to critical theorists—but usually in a manner expunging Heideggerian affinities. For Villa, the chief importance of Heidegger for Arendt's thought resides in the overcoming of metaphysics or the "deconstruction of Western philosophy," a deconstructive strategy that Arendt employed in the effort to develop a theory of action in radical opposition to the tradition of mainstream Western thought and its "antipolitical prejudices." This latter aspect transforms the book's topic into a "three-sided" or three-cornered encounter: involving the juxtaposition of Arendt and Heidegger and the juxtaposition of both to the "foundationalist" tradition.

The book's organization reflects in large measure this three-sided engagement. While the first part of the book profiles the basic features of Arendt's theory of political action, with an emphasis on her radical anti-traditionalism, the second part explores mainly the areas of convergence between Arendt and Heidegger or the impulses that Arendt deliberately appropriated (though, of course, not slavishly) from Heidegger's work. The third part finally proceeds from a more or less amicable juxtaposition to an accentuation of rifts and divergences, specifically to Arendt's critique of Heidegger's "philosophical politics" focused on his alleged "oblivion of *praxis.*" In the first part, the deconstructive élan is directed mainly against a presumed linkage between Arendtian action and Aristotelian praxis, a linkage that has been asserted by numerous writers (from Habermas to Bernstein). In Villa's view (p. 18), although Aristotelian categories provided the "key structure" for her "articulation of the *vita activa,*" Arendt used these categories chiefly to "deconstruct and overcome" the classical framework. Basically, what she pitted against the teleological or "teleocratic" Aristotelian praxis was a conception of action as "self-contained" or as done for its own sake, and of politics as a "performing art" governed by "aesthetic and theatrical metaphorics" (pp. 52–3). Yet this aesthetic turn clearly carried some dangers of which she was aware, especially immoralism and a Nietzsche-style celebration of unlimited performativity governed by will to power. According to Villa, it was her realization of these dangers that prompted Arendt to move to the more-subdued "aesthetic judgment" of Kant's Third Critique, with its emphasis on the spectator detached from immediate impulses. Thus, though indebted to both Nietzsche and Heidegger, Arendt drew the line against a Nietzschean unleashing of agonistics. In Villa's words (p. 81), her conception of action must be viewed "in conjunction with her theory of political judgment," a theory that provides a "forceful critique of the metaphysical and epistemological commitments of Nietzchean aestheticism (the will to power, perspectivism)."

The borrowings or appropriations from Heidegger's work that are developed in the second part mainly involve three thematic areas as unfolding stages of thought: the conception of freedom, the interplay of revealment and concealment, and the "critique of modernity" focused on the rise of subjectivism and technological mastery. Regarding the first topic, Heidegger's ontological construal of freedom constituted—in Villa's account (p. 114)—a "radical shift in paradigm" away from a subject-centered approach, a shift that became "absolutely central" to Arendt's thinking of freedom as a "worldly, tangible reality" (although Heidegger himself "failed" to draw these implications). The topic is closely connected with Heidegger's distinction between "authentic" and "inauthentic" existence, a distinction said to be crucial for an understanding of Arendt's opposition between "work" *(homo faber)* and political "action" in the public realm (p. 136). The deeper ontological interplay of disclosure and concealment (and of world and earth) is found to be reflected in Arendt's juxtaposition of public and private realms and also in her assessment of the progressive "dimming down" of the public space of appearance by the rise of the "social" (p. 146). Regarding the issue of modernity, finally, Arendt followed Heidegger in a number of ways: "I want to show how Arendt's concern for the world and action leads her to appropriate leading themes from Heidegger's critique of modernity, including the subjectification of the real, the 'de-worlding of the world,' and the technological disessencing of our disclosive capacity" (p. 173; although all these themes undergo a "sea-change" as they "migrate from Heidegger to Arendt").

Turning in the third part to fissures and divergences, Villa initially takes exception to overly facile objections to Heidegger's "political philosophy" (as formulated by Habermas and others), objections that reduce his political trajectory to a simple movement from an early voluntarism or "decisionism" to a later phase of quietism or fatalism. For Villa, this portrayal unduly neglects some of Heidegger's genuine contributions, especially the way in which *Being and Time* opened up a "clear role for politics and political speech" (p. 216)—a role Arendt recognized and seized upon in her articulation of the *vita activa.* Curiously, this indebtedness was "dimmed down" or completely dismissed by Arendt in her own later writings, especially in *The Life of the Mind.* As Villa shows, Heidegger at this point appeared to Arendt as a "fundamentally unpolitical" and even "essentially unworldly philosopher," a characterization deriving from his progressive turn to "thinking" and "inner" reflection that ruptured "any relation between the disclosure of Being and politics"; in the case of the later Heidegger, above all, the "withdrawal" from politics and the "world" was "full and complete" (pp. 235, 237). Remonstrating against this simplification, Villa prefers to steer a more cautious path; he finds "irony" in Arendt's portrayal whose own approach "demonstrates how important Heidegger is for thinking of action as freed from the domination of teleology, first principles, and the autonomous subject" (p. 243). For Villa, Heidegger's role in this domain was and remains deeply "ambiguous": his work "radicalizes as well as obliterates *praxis,*" thus contributing both to the recovery and the "oblivion of *praxis.*" This oblivion is basically ascribed to Heidegger's focus on poetic "revealing" or "world disclosure" (in preference to political action). Following largely the interpretation offered by Lacoue-Labarthe, Villa concludes that this focus in the end collapses action into artistic "making" or production, and politics into a kind of "plastic art" governed by a holistic telos. Despite his attempted overcoming of (teleological) metaphysics, Heidegger thus remained "caught within the productionist paradigm"; even in his later reflections on *Gestell,* he effaced politics "through the figure of poetry" (pp. 248, 253).

Arendt and Heidegger is an intellectual tour de force, ranging over a vast terrain of primary and secondary literature. Both in the subtlety of its argument and its lucidity of style, the book deserves high praise—though, of course, not uncritical praise. Some (relatively minor) qualms have to do with occasional rhetorical flourishes and with the steadiness of the balancing act performed throughout the study. At some points, as a counterfoil to Arendt's radicalism, Heidegger is charged with "cultural conservatism"—seemingly out of the blue. Thus, Arendt is credited with extracting "novel and unexpected *political* implications" from a Heideggerian stance "mixed in cultural conservatism" (p. 173). Elsewhere we learn that such a stance "yearns for the rootedness of a premodern existence (for example, Heidegger at his most simplistic and loathsome, or Pope John Paul II)" (p. 204). Aside from such obiter dicta, the mentioned balancing act gives cause for concern, especially in the two concluding chapters where Villa both agrees and disagrees with so many authors as to render his path a bit tortuous. Thus, although generally critical of Habermasian consensualism, he also notes that Habermas "does succeed in presenting Heidegger as a dubious resource for the theoretical consideration of politics" (p. 230). Similarly, despite reservations about both Habermas's and Arendt's accounts, we read that, generally speaking, their critiques are "persuasive, revealing fundamental reasons for the underdevelopment of the concept of action in Heidegger" (p. 241). The same pattern emerges with respect to numerous other interpreters. Thus, while often reluctant to embrace their views, Villa nonetheless holds that, in their insistence on praxis, both Bernstein and Taminiaux are "on the right track" (p. 262). In the end, Villa chooses to steer "a middle course between Schürmann and Bernstein"—a precarious course indeed—by agreeing with the former that "the later Heidegger radicalizes the question of action by interrogating the metaphysical construction of *praxis,*" while also agreeing with Bernstein that "the discourse on technology proves insufficient, finally, for a genuine rethinking of action" (p. 262).

A more important issue for critical reflection is the status of Arendt's theoretical position—whose plausibility and internal coherence Villa does not sufficiently explore. My point here is not to rehearse familiar questions such as these: Is it really possible, in Arendt's categorical fashion, to segregate the domains of labor, work, and action (especially since Heidegger nowhere "segregated" authentic from inauthentic existence or revelation from concealment)? Does a radically "self-contained" action not readily slide into pointless action or action for action's sake, and does the accent on theatrical performance not open the door to the kind of "media spectacle" into which American politics has largely turned? In the context of Villa's study, such questions must yield to other queries. Is it really possible, in the way Villa maintains, to separate Arendt's aesthetic approach to action from Nietzsche's "aestheticism" and agonistics—without undercutting Arendt's proclaimed radicalism? Moreover, does her recourse to Kant's Third Critique really provide a suitable demarcation line, and at what cost? Can one quite so smoothly blend Kant's stress on the spectator with the celebration of the *vita activa?* Villa himself seems to harbor doubts as he speaks alternately about Arendt's "limited Kantianism" and her resolute move "beyond Kant and Aristotle" (pp. 50, 77). The book's focus generates this additional query: how to reconcile her Kantianism (even limited) with her indebtedness to Heidegger (not to mention her debt to Jaspers whose role is understated in this context)? Villa affirms at the end that "Arendt's project remains *irreducible* to Heidegger's" (p. 265); quite so. But this also means that Arendt's thought is beleaguered by problems that are not the same as Heidegger's.

Villa's statement can also be reversed. If the book implies that Arendt extracted all the valuable aspects of Heidegger's work leaving behind only the dross, I would want to demur. Arendt developed *one* interpretation of Heidegger's contribution to action; Schürmann developed another (which deserves more attention). But the field is still quite open for other ventures. Yet, despite mutual irreducibility, there is a sense in which Arendt's thought was more dependent on Heidegger than she herself was willing to acknowledge—and than Villa grants. The point has to do with poetic world disclosure. For Villa, it was Heidegger's accent on this domain that caused him to slide back into the teleocratic "production paradigm"—a strange claim that trades on the complex and nearly untranslatable character of the Greek terms *techne* and *poiesis.* (Heidegger has spent much time showing the distance of these terms from modern technology.) *Pace* Lacoue-Labarthe, what Heidegger's comments on Hölderlin, Trakl, or George have to do with "plastic art" as a form of "making" or fabrication is unintelligible. Once this claim is set aside, the significance of poetic world disclosure for Arendtian praxis comes more clearly into view. Villa makes much of the different levels of Heideggerian and Arendtian agonistics. Thus, he finds in Heidegger a "struggle between being and appearance" and a privileging of "poetic, world-disclosive activity" that downgrades "the *praxis* of the many"; in fact, Heidegger's "poetic agon" regarding the "truth of Being" is said to be severed from the "agonistic sharing of words and deeds" in a public world (pp. 154–5). But here again the issue can be reversed.

Cut loose from the ontological moorings of Heideggerian agonistics of world-disclosure, Arendt's claimed "performance ontology" makes no sense and is in danger of sliding into a shallow pragmatism of play-acting and routine chatter (that may be why her thought seems so "ready-to-hand" to American pragmatists). What is at stake here is primarily the vaunted novelty or innovative capacity of Arendtian action. For what is the source of this novelty—given that deeds are hedged in by normalizing conventions and words by language games? As Villa notes, in the public realm we are liable to "play established, familiar language games, with evolving but fairly strict rules about what counts as an acceptable move or meaningful utterance" (p. 227). To ascribe novelty to birth or "natality" seems awkward in Arendt's scheme since birth belongs to the domain of labor and hence unfreedom. Although uncomfortable with any reliance on "faith," Villa himself speaks of "the 'miracle' of spontaneous initiatory action"—a miracle made all the more mysterious by Arendt's later suspicion that action in the strict sense is "no longer possible" (pp. 266, 269). Do considerations of this kind not lend ample support to Heidegger's insistence on world disclosure, on the poetic opening of a space for human dwelling (including political dwelling)? Seen from this angle, Heidegger's is not a "worldless" but certainly an "untimely" thinking, a thinking that is always yet "to come."

Reconsidering American Liberalism: The Troubled Odyssey of the Liberal Idea. By James P. Young. Boulder, CO: Westview Press, 1996. 437p. $65.00 cloth, $20.00 paper.

Thomas L. Dumm, *Amherst College*

James P. Young's reconsideration of American liberalism is a comprehensive and thematic re-reading, not only of the liberal idea in America, but of the history of most of American political thought from the colonial period to the present. Concerned with the current status and future prospects of the liberal idea, Young presents a history of the present of liberalism that revises the great Hartzian thesis of liberal consensus by placing the Puritan tradition as the major opposition voice to liberalism from the beginning of the English settlements in North America to the present. For Young, Puritanism, with its emphases on both voluntarism and hierarchy, individualism and fate, has inspired the legacies of Jacksonian democracy, abolitionism, feminism, and comunitarianism in turn, as complicatedly as these movements may have departed from Puritanism. Along with minor vestiges of feudalism that survived from the colonial period and the trans-Atlantic circulation of the ideas of neoclassical republicanism, Puritanism constitutes what might be called the oppositional imagination of liberalism, belying Hartz's idea of a liberal consensus that was so singularly unchallenged as to constitute a hegemonic force in American political culture.

One risks committing the error of oversimplification when briefly reviewing as sweeping and thoroughly revisionist a study as this. Indeed, one needs to warn the reader that any sketch of this book can only serve as a gross reduction of a very carefully rendered reading of the main threads of American political thought. Young does not oversimplify. His core argument with Hartz consists of the idea that if one is to access adequately the power of liberalism through the course of American history, one is compelled to complicate the narrative of liberalism to account for the opponents it has attracted (and sometimes produced) and to appreciate the nuance of the core ideas of liberalism and of its opponents, even the nuances of the great simplifiers (such as Hartz himself). He draws upon the scholarship of hundreds of thinkers, major and minor, to trace the thread of liberalism's contested history. By placing liberalism on the field of contested ideas and within the struggles in American history over slavery, free labor, segregation, communism, gender equality, and multiculturalism, he shows how liberalism itself is a living theory, not a rigid set of formulas unresponsive to the challenges it has faced, nor merely "enduring."

Young retrieves the Puritan opposition both to illuminate the various minority voices of American thought that have been swept aside or ignored by the Hartzian-inspired liberal literature of American exceptionalism and to connect the strands of various contemporary critiques of liberalism together so that they might better illuminate the conflictual character of liberalism's history. This is a very ambitious agenda and one that for the most part succeeds. And even when Young reaches conclusions that one might argue with, he provides readers with the information needed to understand the perspective on whatever school of thought he is explaining.

While historical in perspective, this study is essentially an argument about the emergence of the contemporary embattled state of liberalism. For Young, the post–World War II era of liberalism is perhaps its most endangered period. The challenge to fulfill the promise of rights to minorities and women has generated reactions on both the right and left and has resulted in a liberalism that seems on the defensive. In an argument that echoes E. J. Dionne's (1991) *Why Americans Hate Politics,* but which deepens Dionne's study of the rise of the New Right and the New Left, Young suggests that a liberalism that finds its primary means of responding to injustice through the extension of rights may have become unable to acknowledge the myriad problems of the late modern era. Noting the rise of a kind of identity politics that edges into fragmentation and balkanization, on the one hand, and a communitarianism that sometimes edges into the simplicity of reactionary conservativism, on the other, he suggests that contemporary liberals need to rethink the repertoire of means available to advance the ends of individual dignity and freedom.

In a study of this scope there are always details that one might contest. For this reader, the arguments Young makes concerning multiculturalism and what he characterizes as postmodernism seem off balance. In criticizing multiculturalists, Young seems less attuned to the fears of powerlessness that gives rise to some of the more immoderate statements of Afrocentric thinkers and radical feminists, especially compared to the sympathy he expresses for more reactionary thinkers of the Right. While Young gives face value to the argument that the greater danger to liberalism is posed by the New Right, his argument in the last third of the book devotes equal attention to both muliculturalists and reactionary conservatives, ignoring the power differential between the two. And, while at least acknowledging the existence of those he calls postmodernists, Young too easily embraces the characterization of these thinkers as posing a rejectionist critique of modernity and hence of the most cherished institutions of American liberalism.

But these contestable exclusions and characterizations aside, this is a valuable work. In laying down the story of liberalism in the United States as a complicated and conflictual narrative throughout its entire history, Young has performed an important service for all serious students of American political thought. For its footnotes alone this book could serve as the most important single text used in graduate seminars in American political thought for years to come. Browsing through those notes (which is still the best way to determine the quality of a work of this genre), one comes away with deep admiration for the author's energy and patience and for the deep love for the culture that must have inspired Young as he labored over the years. *Reconsidering American Liberalism* is an admirable book.

American Politics

Freeing the First Amendment: Critical Perspectives on Freedom of Expression. Edited by David S. Allen and Robert Jensen. New York: New York University Press, 1995. 300p. $50.00 cloth, $18.50 paper.

Donald A. Downs, *University of Wisconsin, Madison*

In the first major essay of *Freeing the First Amendment,* noted First Amendment scholar Frederick Schauer declares that vibrant criticism of current liberal free speech doctrine is needed because "free speech itself, has become the orthodoxy, or ideology" (p. 11). Although Schauer and the other writers in this volume oddly or conveniently ignore the widespread threats to academic freedom and free speech that have erupted on college campuses and elsewhere in the name of political correctness and other anti-speech orthodoxies, two points are clear. First, liberal free speech orthodoxy did prevail in the intellectual world (if not elsewhere) until things got interesting with the eruption of the Skokie controversy in the late 1970s. Second, as Schauer points out in the spirit of John Stuart Mill, all opinions, including those in favor of free speech, become "dead dogma" if they are not challenged (p. 21).

The essays in this volume are representative of the major criticisms of free speech generated from the Left. In general, they deal with epistemological objections, objections concerning social justice, and those pertaining to citizenship. The essays are a mixed bag in terms of quality and effectiveness. Some constitute orthodoxy in their own right, whereas others bear the potential to persuade because they struggle with the First Amendment dialectically, rather than dismissively.

The weakest articles in this book are those dealing with social justice. They are weak not because of the just causes for which the authors stand, but because the accounts fail to grapple with important or obvious counterarguments. In what amounts to a restatement of the theory of Catharine MacKinnon, Robert Jensen, and Elvia R. Arriola criticize liberal doctrine's protection of most forms of pornography. Pornography's display constitutes discrimination per se. Present doctrine's distinction between speech and action "is of questionable value" (p. 212). But completely forsaking the speech/action distinction (which is a policy prescription, not a doctrine based on any particular claim about reality) is synonymous with abandoning free speech, for such a reform provides a rationale for restricting speech that anyone finds troubling or simply annoying. And such a logic presumes that the targets of speech are typically helpless victims rather than active citizens who are worthy of respect. Similar problems beset Robin D. Barnes's essay in favor of broad limitations to hate speech, though Barnes does draw a balance that is more judicious than Jensen and Arriola (Barnes would permit hate speech in private forums and prohibit it in public ones). As Owen Fiss (in a separate article) and Barnes show, certain forms of targeted hate speech can be very harmful, calling for proper prohibition. But cannot present doctrine (which does not protect "fighting words" and meaningful threats) accommodate these claims without throwing the baby out with the bath water? Finally, Victoria Smith Holden argues that the First Amendment and the Thirteenth Amendment (anti-slavery) should be reinterpreted to give "effective voice" to workers in corporations, including the rights to criticize the boss and the company, to maintain jobs and advancement, and to participate in strategic decisions. While this essay deals with issues of class that are strangely ignored in public law today, Holden nowhere anticipates critics who will have obvious questions about such wholesale refashioning of the Constitution and the role of the courts in the polity.

The epistemological critiques accuse First Amendment doctrine and theory of being beholden to outmoded assumptions about language and truth. Thomas Streetor contends that the "linguistic turn" in philosophy reveals that reason and truth are essentially arbitrary, thereby rendering naïve present doctrine's belief in a special realm (the marketplace of ideas, the public forum, etc.) in which the sifting and winnowing of ideas takes place. Sandra Braman shows how the historical evolution of the Supreme Court's cases concerning the regulation of product labeling and intellectual property parallel Baudrillard's portrayal of the movement away from a "referential" theory of knowledge toward "the hyperreal aspects of the postmodern condition" (p. 190). Norman Rosenberg demonstrates from another postmodern perspective how certain film noir portrayals of the legal system throw doubt upon the assumption that "more speech" is the antidote to "bad speech." The films "highlight the degree to which speech acts, even within law's own majestic empire, are located in complex networks of power" (p. 157).

The epistemological challenge should awaken referentialists from their dogmatic slumbers. But as Paul Chevigny (*More Speech: Dialogue Rights and Modern Liberty*, 1988) and other practitioners of the linguistic turn have shown, such theories can be used to promote free speech positions as readily as censorship. Assuming for the moment that all truth lies in language, then what grounds exist for limiting the flow of discourse? Dropping the assumption just made, more thinkers are starting to question the epistemological, ontological, and ethical implications of radical denials of referential reality. Did the Holocaust occur in "facticity" (Braman's dismissive terminology) or is it simply the product of language and convention, as the Holocaust deniers claim (Deborah Lipstadt, *Denying the Holocaust: The Growing Assault on Truth and Memory*, 1993)?

Critics stand on more solid ground when they evince critical commitment to reason, common sense, and courage—the qualities Hannah Arendt shows are essential to citizenship and public life (*The Human Condition*, 1958). The best essays in *Freeing the First Amendment* take free speech seriously while criticizing present law for not reinforcing the qualities of active citizenship. First Amendment doctrine is too indebted to the notion of citizens as passive, individualistic consumers of information. Drawing on the theories of Arendt and Jürgen Habermas, David S. Allen wants the Supreme Court to give more weight to citizens' rights to acquire information by their own efforts. Cass Sunstein's proposals for a "new deal" for free speech entail more Federal Communications Commission regulation in the name of fostering a more diverse and qualitative array of programming. The proposals are pocked with problems that critics have exposed (why should we trust the benevolent state to act wisely in this domain, especially given how politicized debates over free speech have become?), but Sunstein takes freedom seriously and hopes that his proposals will contribute to "the considered judgments of a democratic polity" (p. 71). Unlike most of the other writers, Sunstein and Allen actually argue for more speech rather than less, and they give citizens credit for having strong minds.

The principle of active citizenship combines commitment to free speech with political and moral courage and obligation. Paul Siegel, a gay activist who writes the only essay favoring standard free speech logic, shows how groups outside the mainstream of power forsake free speech at their peril. Gays and lesbians are better served by liberal free speech doctrine for the simple reason that it gives them opportunities to organize and make their cases to the public. The strength of the gay and lesbian threat to the established order "depends upon our willingness to speak out, to show our recognition that we are in fact, a free speech movement" (p. 245).

Battles on the Bench: Conflict Inside the Supreme Court. By Phillip J. Cooper. Lawrence: University Press of Kansas, 1995. 224p. $24.95.

Lawrence Baum, *Ohio State University*

No group of people who work together on consequential matters for long periods can avoid conflict. For that reason, the frequency with which conflicts arise among justices on the United States Supreme Court and the seriousness of some of these conflicts are hardly surprising. Yet observers of the Supreme Court have always been intrigued by conflicts within the Court, in part because those conflicts often are at variance with the Court's own preferred image of high-minded collective efforts to find the best interpretations of the law. Conflict is one important concern or theme in much of the research of the Court, particularly in historical and biographical studies. In *Battles on the Bench*, Phillip Cooper focuses directly on conflict as an aspect of life and work in the Supreme Court—though, inevitably, his book deals with other aspects of the Court as well.

His book examines four questions: "Why do the justices fight? How do they fight? What difference does it make? Why do they not fight more often?" (p. 3). To answer these questions, Cooper draws evidence from biographies of justices and other scholarly research as well as interviews and the papers of several justices. One motivation for his writing this book was his belief that insufficient use has been made of the primary source materials available in biographies.

The book's approach to the four questions and to the examination of conflict in general is informal, an informality symbolized by the absence of an explicit definition of *conflict*. The book does not analyze issues within theoretical frameworks. Instead, Cooper answers his questions with typologies or lists of relevant factors that appear to be derived inductively from evidence on conflict in the Court. In turn, that evidence is used primarily for illustrative purposes rather than as a means to test the validity of the propositions that Cooper presents; there are no tests of hypotheses. In line with this informal approach, the book does not directly address broad theoretical issues in the study of courts.

Cooper answers the "how" question with a typology of conflicts based on two dichotomies: whether conflicts are personal or professional in their terms and whether they are internal (kept within the Court) or external (made public). This typology is used as a means to distinguish among forms of conflict that may differ considerably in their sources and impact.

Addressing the "why" question, the book cites several reasons for conflict among the justices. The reasons that it offers are diverse, but primary emphasis is given to disagreements about legal policy. For instance, Cooper notes that ideological change resulting from new appointments to the Court creates the potential for serious conflict. However, he also points to sources of conflict that are less closely tied to disagreement about policy, including rivalries over promotion to chief justice and "ego clashes."

Cooper believes that conflict among the justices has been relatively limited, especially in comparison with conflicts within other government institutions. "In the final analysis, what is amazing about the Court is not that there are conflicts, but that there are not more of them and that the clashes that do arise are handled, with some exceptions, so well" (p. 152). He ascribes this limited conflict in part to the Court's work patterns and to the effectiveness of some chief justices. But he gives primary emphasis to a range of institutional norms, many of them reflecting the justices' awareness that they and the Court benefit if conflicts are kept within bounds. His analysis of the reasons why conflict is not more extensive and serious is one of the most interesting and distinctive elements of the book.

The book gives attention to temporal variation in the extent and form of conflict. Indeed, Cooper points to what he views as an important recent change in the Court's group life, one in which justices increasingly air personal conflicts in public forums. Perhaps the primary forum is concurring and dissenting opinions, in which Cooper sees a movement over time to increasingly direct attacks on colleagues.

In Cooper's view, conflict is not just an interesting element of Supreme Court behavior; it has significant consequences. Conflict among justices can affect their inclination to leave the Court, and it helps to shape views of the Court among its audiences. It also can have a number of effects on the Court's decision-making process, most of which involve constraints on collegial decision making. For instance, Cooper argues that one justice can become a "reliable anti-authority" (p. 133) for another. He sees personal conflict that is made public as most consequential for the Court's functioning and its image. For this reason he is troubled by what he perceives as recent growth in the frequency of such conflict.

Battles on the Bench discusses Supreme Court conflict in a readable style. It presents an impressive volume and range of information about conflicts in the recent and more distant past and a number of interesting ideas about conflict. With its informal approach, the book does not provide systematic evidence about the incidence and forms of conflict or about its causes and consequences. But by pointing to possible patterns involving conflict on the Court, the book facilitates inquiries designed to produce such evidence. Many of its propositions, for instance, provide the basis for hypotheses that can be tested through qualitative or quantitative analysis. And scholars who are interested in theoretical issues concerning the explanation of judicial behavior will find in the book a good deal of material relevant to those issues, material that can be useful in thinking about the validity of competing explanations.

Ethnic Ironies: Latino Politics in the 1992 Elections. Edited by Rodolfo O. de la Garza and Louis DeSipio. Boulder, CO: Westview, 1996. 225p. $55.00 cloth, $18.95 paper.

Roberto E. Villarreal, *University of Texas at El Paso*

Ethnic Ironies: Latino Politics in the 1992 Elections is a continuation of an effort to establish a record of Latinos' participation in the electoral system. In a way, it is a comparative study of national, state, and local elections during the 1980s and early 1990s. This edited volume provides readers with an historical account of electoral behavior. More specifically, the scholars describe and analyze not only the role of the Latino electorate, but also the means of measuring Latino influence and the multitude of political ironies. Moreover, it examines ethnic politics from the top down (elite) and from bottom up (mass).

Demographic characteristics are key elements in electoral participation. The majority of Latinos are Mexican American and Puerto Rican, and for the most part they are younger, poorer, and less educated than other ethnic groups. Mexican Americans and Puerto Ricans have also experienced a far greater rate of noncitizenship. The effects of the Latino "noncitizen" are most significant in determining political influence, and its impact varies from state to state, from an insignificant percentage in some states to as much as 50% in California. The geographic concentration of Latinos in nine large states, which contain 75% of the electoral vote to win the presidency, is another demographic trait that can play a significant role in close elections. These characteristics predominate in the discourse of this volume.

Demographic characteristics and the response by Latinos and non-Latinos are the sources of ethnic ironies. Two major ironies stand out in the election of 1992: first, while the turnout among all voters *increased* over the previous election (1988), the increase in Latino voters in 1992 "did not match the growth rates achieved between 1980 and 1988 and barely kept pace with the growth in the Latino adult population" (p. 29). Second, if there was any significant impact, it did not come from the large states with heavy Latino populations; instead, it came from the small states.

The authors return to the questions of when Latinos can and do influence candidate selection and electoral outcomes. As a way of measuring the political influence of Latinos in the electoral process, Guerra and Fraga's study of the 1992 California elections identified a number of conditions necessary for the creation of an effective Latino electorate. These conditions fall into two categories: *contextual*—conditions outside the realm of control of Latinos—and *strategic*—conditions within the realm of Latino influence. The contextual and strategic model serves three fundamental purposes: it can possibly predict the effectiveness of the statewide Latino vote before an election; it can explain the effectiveness of the vote during an election; and it could provide direction to those interested in trying to develop new approaches to enhancing the influence of Latino voters. The contextual and strategic model examines the influence of the Latino electorate and develops a path for greater political participation. Yet the lack of an effective national strategy diminishes any well-intended approach. Are we asking the right questions? Do we really understand and recognize the complexities of Latino electoral participation? Falcon's frank statement that "the ethnic and political diversity of the Latino population defies generalization and continually pushes researchers to deconstruct that population at the local level" (p. 185) is a significant consideration in the formulation of new strategies.

To test this model, the volume contains eight state-level analyses by experts in their respective states and a chapter that synthesizes and integrates the findings of these case studies. Garcia's study of New Mexican politics in the 1992 elections is almost defined by the title of the chapter, "Conventional Politics under Unusual Circumstances." A salient issue in this study is that Hispanics are much more mainstream and less distinctive than other case studies have suggested. Any direct effort to focus on Hispanics was "primarily because they were traditionally strong supporters of Democratic candidates" (p. 71). In the state of Colorado, Hero reports that, while Latino votes were an important part of the Clinton victory, his presidential campaign approach was not a distinct "Hispanic" message at all; rather, it dealt with general issues that concern Latinos. In Arizona, Avalos contends, the Latino voice was silenced. Ironically, Latinos in that state voted in higher rates in the 1992 presidential election but did not alter electoral precedent. Arizona went Republican and the majority of the Latinos voted Democratic. The "missed opportunity" to affect national politics is based on Latinos' persistently low voter turnout and the traditional state Democratic Party indifference to the Latino voter. Martinez's "Unrealized Expectations" maintains that the Latino population in Texas has essentially provided the "raw material for greater political influence . . . and strengthened the constitutional argument for greater representation based on population" (p. 128). Nevertheless, these raw numbers did not transfer into political influence. There was, however, a sizeable increase in the number of Hispanic state legislators. The message seems clear: Hispanic leaders must pay greater attention to municipal and state elections if they are to have more political influence.

The last part of the book focuses on three "new kids on the block states" (Illinois, Florida, and New York) where Latinos can obtain a level of electorate influence. Rey's study of Illinois in the 1992 election "reinforces previous analysis that the likelihood that Latino voters will exercise a decisive influence (swing voters) at either the state or national level is remote for the foreseeable future" (p. 165). Nevertheless, some leverage can be maintained at the municipal level. Moreno and Warren's study of Florida's Cuban Americans presents a very different electoral picture. Through Republican Party politics they have demonstrated strong group cohesion and mobilization in support of national and state candidates. They have made a strong case for elite politics and are consistently building on their "electoral clout to project themselves into those institutional processes that not only respond to narrow group interests, but more broadly shape political and policy agendas at all levels of government" (p. 183). Although the group's activity fits Guerra and Fraga's model, there are signs on the horizon that Cuban Americans are becoming more diversified and less cohesive along party lines as well as on policy issues and ideological orientation. Finally, Falcon provides a narrative account of political events of Puerto Ricans in New York and presents the most realistic picture of ethnic electoral politics in this volume. His characterization of Puerto Ricans leaves the reader with the impression that inclusion is an impossibility for a growing number of the poor. As a result, ethnic elites get involved in quarrelsome arguments to the point of destruction. The Puerto Rican poor cannot rely on elected officials, nor can they rely on electoral politics over more direct participation. One can only agree with the author that "for the vast majority of Puerto Ricans, it was just something they saw playing on television, no more significant than the *Love Connection* or *Tres Destinos*" (p. 205).

Ethnic Ironies contributes greatly to the literature on political participation. Yet it has limited value to Latino and non-Latino policymakers. There are a number of questions and conditions that need to be addressed, including some beyond the electoral process. For those of us, for example, who have experience in both scholarly and practical politics, it seems clear that *Ethnic Ironies* is limited by our research tools and some traditions of the profession. Moreover, the concept of the "potential perpetual" voter is problematic and a diluting factor that needs to be addressed by scholars and policymakers. Latino political strength lies in its numbers. But this "potential" influence often threatens dominant and powerful groups who are determined to manipulate the electorate to win and keep control. Another area of concern is ethnic ambiguity. Moreno and Warren warn the reader that there is ambiguity in the "debate about whether there is a Latino politics in the United States that is both identifiable and separable from the politics of other groups or from that of the nation as a whole" (p. 169). There is a major effort to integrate Latinos in the political system as a way to gain influence. What happens with the "mainstreaming" of Latinos in places like New Mexico or Florida where the predominant thinking is that "Hispanics are just like everyone else" (p. 55)? Does ethnic cultural identity translate into political events? Finally, electing Latinos (and this applies to any other group) gives us no assurance that they will work for the interests of their constituents, particularly when the political culture continues to change and is not conducive to such behavior. Political culture and ethnic ambiguity are critical subjects for future debate and discussion.

The Rhetoric of Church and State. By Frederick Mark Gedicks. Durham, NC: Duke University Press, 1995. 196p. $49.95 cloth, $18.95 paper.

Stephen V. Monsma, *Pepperdine University*

In *The Rhetoric of Church and State* Federick Gedicks powerfully analyzes the underlying causes of the controversies—and many would say incoherence—that often mark the Supreme Court's church-state jurisprudence. Gedicks begins by citing examples both of persons who have argued that the Court's church-state stance is hostile to religion and also of other persons—equally sincere and equally able—who reject and are puzzled by such allegations. He goes on to suggest that these contradictory conclusions can only be explained by the lenses—or set of assumptions and theories—through which different persons are viewing the Court's jurisprudence. In keeping with postmodern thinking, Gedicks argues that "one's theoretical preconceptions of the world alter his or her intuitive experience of it" (p. 9).

Throughout the nineteenth century the culturally dominate mindset—and the discourse that flowed from it—was that of religious communitarianism, which saw the state as appropriately supporting majoritarian Protestant values as the foundation for a strong, moral society. In the twentieth century this mindset was challenged by, and largely replaced by, secular individualist discourse, which stresses individual choice and the relegation of religion to the private sphere. Gedicks does not simply defend either one or the other of the discourses he describes. He is very tough on secular individualist discourse and the way it has been used by the Supreme Court, but he also makes clear he is not calling for a return to nineteenth-century religious communitarian discourse and the resulting de facto Protestant establishment.

Gedicks has made a major contribution to the ongoing discussion of church-state issues. Studies continue to be published that analyze Supreme Court church-state decisions and condemn or commend them. But Gedicks moves beyond this level by seeking to understand why the Supreme Court approaches church-state issues as it does. In making a distinction between secular individualist discourse and religious communitarian discourse he offers a theory that significantly aids our understanding of the confusing world of church-state law. By doing so, he provides the basis for a more telling critique of current church-state interpretations than is the case among commentators simply defending the strict separationist or accommodationist perspective.

Especially helpful is Gedicks's claim that although secular individualist discourse is now dominant on the Supreme Court and among American elites, it has not fully replaced religious communitarian thinking, with the result that decisions are sometimes a confusing mixture of religious communitarian thought supported by secular individualist reasoning. Thus, Gedicks asserts that the Supreme Court, in holding aid to parochial schools unconstitutional and aid to religious colleges constitutional, was justifying "results that are possible only within one discourse with arguments that are coherent only within the other" (p. 84). Religious communitarian discourse would allow aid to religious colleges, but the Court decided, under secular individualist discourse, that aid to parochial schools was unconstitutional. Thus, the only way it could reach the desired result in the case of such colleges was to do violence to their true nature by denying their religious character, thereby fitting their receipt of aid into secular individualist discourse.

Gedicks also sees substantive problems in secular individualist discourse, especially due to its relegation of religion to the private sphere of human existence. He writes: "Secular individualism's confinement of religious belief and action to private life is neutral between religion and nonreligion only if one can demonstrate the undemonstrable—that religion is inherently and intrinsically private" (p. 119). For example, Gedicks argues that the blindness of secular individualist discourse to the public facet of religion led it to hold that public funding of parochial schools is unconstitutional due to its violating the norm of neutrality. If religion is a purely private concern and if in fact parochial schools are pervasively religious, to aid them is to aid religion, thereby giving a force with no legitimate public role a public advantage. But Gedicks goes on to point out that under secular individualism the Supreme Court, also in the name of neutrality, has ruled in favor of the constitutionality of public support for the education of *individuals* in religious schools (*Mueller v. Allen*, *Witters*, and *Zobrest*). Secular individualism encourages the Court to see the importance of neutrally allowing benefits to all persons, religious and nonreligious alike. Because secular individualist discourse sees religion as being purely private in nature, it defines neutrality differently in different situations. The problem is that if denying religious persons' participation in public programs of aid violates neutrality, neutrality would also appear to be violated by not providing funds for parochial schools while providing copious funds for secular, public schools.

I believe Gedicks's secular individualist-religious communitarian distinction works better in the case of establishment than in the analysis of free exercise decisions. Gedicks argues that in free exercise jurisprudence the Supreme Court has used secular individualist discourse to reach religious communitarian results. Since the *Smith* decision in 1990 the Court has virtually ended free exercise protections, leaving religious freedom up to legislative majorities. It has thereby furthered religious communitarian values. But it continues to use secular individualist justifications for these decisions on the basis that religion ought not to confer special benefits, as would be the case if religious persons were ruled exempt from the legal responsibilities that all others are under. But as Gedicks himself acknowledges, secular individualist discourse can also be used to support the compelling state interest test articulated by the Court in *Sherbert* and *Yoder* and later abandoned in *Smith*. Under those Supreme Court interpretations, individuals were left free to make their own religious choices and those choices were seen as inconsequential for the public realm since religion was assumed to be a private matter.

The issue this book does not address is what sort of discourse should replace religious communitarian and secular individualist discourse. Gedicks makes a powerful case of their need to be replaced, but leaves the question of what to replace them with to other scholars and—one would hope—to his future efforts. Even those readers who do not agree with all of Gedicks's analyses will find their insight into the continuing church-state struggles in the courts and in society deepened.

The Federal Impeachment Process: A Constitutional and Historical Analysis. By Michael J. Gerhardt. Princeton: Princeton University Press, 1996. 233p. $29.95.

Mark J. Rozell, *American University*

It is no accident that commentators and scholars pay the most attention to the impeachment process during periods of official proceedings against high-level federal officeholders. The topic has less obvious appeal during times when there is little or no such controversy. Consequently, the formal study of impeachment by constitutional scholars and political scientists has recently been somewhat neglected. Gerhardt sets out to provide a corrective to this gap in the literature and fulfills the task admirably in this volume, which is both concise and comprehensive in its coverage of the topic.

Although there has been a good deal of scholarly study of the process of disciplining and removing federal judges, this book is the first comprehensive examination of impeachment since the publication of Raoul Berger's (1973) leading study *Impeachment: The Constitutional Problems*. Studies from that period focused on the issues surrounding presidential impeachment; Gerhardt focuses more on judicial impeachments and the implications of Congress's nonreviewable power.

The book is nicely organized, beginning with an overview of the historical origins of impeachment and then covering historical trends, constitutional ambiguities and interpretations, proposed reforms, and the author's brief analysis of how well the impeachment process has worked historically. Gerhardt relies on a variety of useful resources including the writings of the constitutional framers, original committee reports and transcripts, public statements, Senate votes in impeachment trials, public testimony, and a limited number of survey responses from members of Congress. The somewhat weak survey response clearly disappointed the author but did not compromise the value of his overall thorough research.

The first section covers the constitutional convention and state ratification debates over impeachment. Drawing from British experiences, writings of the constitutional framers, and state ratification debates, Gerhardt traces the formation and development of the "uniquely American" (p. 11) approach to impeachment, wherein an electorally accountable institution—Congress—was granted this authority. The second section focuses on the basic process of impeachment proceedings in the House and the Senate and the difficulties that result from the formal rules. It is here that the author clarifies why the text of the Constitution does not provide adequate answers to many of the questions that arise concerning the impeachment process. Relying on a variety of sources, Gerhardt seeks to provide some guidance to answer these questions in the third section. He devotes the final section to an analysis of various reforms that have been proposed to alleviate ambiguities and inconsistencies in the impeachment process. He is cautious about the viability of any of the major proposed changes.

Gerhardt concludes that, despite the constitutional ambiguities and historically stated concerns about Congress's fitness to conduct impeachment proceedings, the process has generally worked quite well. It has not been widely used for partisan purposes, legislators have undertaken the task of impeachment with serious intent and have been suitably cautious about the use of that power.

Here some may disagree, for the author believes that the relative lack of impeachment proceedings in U.S. history is proof that the process has worked. Alternatively, Congress may be too cautious, having refused to use its most formidable check against the executive branch to the point of nearly rendering the impeachment power ineffective. Although Congress has generally avoided partisan impeachment proceedings, partisan concerns (e.g., the popularity of a sitting president) may prevent legislators from exercising this check when it may be appropriate.

Gerhardt closes with some of his own sensible recommendations for Congress to improve its handling of impeachment procedures. For the most part, these proposals are geared toward making the current system more efficient (e.g., streamline fact-finding, create new committees, publish a manual on precedents and procedures), rather than to address some of the more substantive faults that he identifies earlier in the book. He clearly prefers to tinker with the existing system than to reform it, given his general admiration for how the process usually works.

If for no other reason, this volume is worthwhile because of its clear and comprehensive examination of a somewhat neglected topic. Those seeking a broad overview of the history and issues surrounding the impeachment process could not do any better than to consult this book. When read alongside Berger's earlier study, students of constitutional law will have exhausted the essential material on the topic of impeachment. Ultimately, however, this book offers more, for it provides the kind of historical and constitutional analysis of American institutions that once guided much of the field but is nearly lacking in political science today. This book should therefore appeal primarily to scholars of American institutions generally and constitutional law more specifically.

Strategic Disagreement: Stalemate in American Politics. By John B. Gilmour. Pittsburgh: University of Pittsburgh Press, 1995. 200p. $49.95 cloth, $14.95 paper.

Sean Q. Kelly, *East Carolina University*

In *Strategic Disagreement*, John Gilmour provides a well-structured discussion of the political strategies pursued by politicians and political parties that often result in stalemate in American politics. This work is particularly interesting within the political context of the past several years. Elected promising to reform health care and the welfare system, Democratic President Bill Clinton was unable to do either despite Democratic control of Congress. As I write, the Democratic President and Republican Congress are yet to reach agreement over a budget for FY 1996, shutting down the federal government three times and edging the country close to default. The marketing director at Pittsburgh Press could not have planned better conditions for the book's release.

Gilmour builds much of his analysis on the premise that the two political parties have relative advantages in different policy areas: Democrats are perceived as strong supporters and defenders of social programs, the Republicans as strong supporters of national defense and exponents of fiscal restraint. Republicans are "disadvantaged" in relation to social legislation; Democrats are disadvantaged concerning defense and fiscal issues. Faced with the necessity of passing high-profile legislation in various policy areas—health care, social security, defense, tax policy, and so forth—the political parties may prefer to avoid compromise and political *agreement* for several reasons. Strategic politicians may wish to avoid vociferous and damaging criticism from constituent groups who prefer ideological purity to compromise; may prefer to maintain their policy distinctiveness compared with the opposition party, thus preserving their political advantage in the policy area; or may calculate that compromising halfway in the present may make complete victory impossible in the future.

Much of the rest of Gilmour's book explains the distinctive strategies that politicians pursue that make stalemate an ever-present feature of contemporary American politics. Which of these strategies is pursued depends on the degree of (dis)agreement over the desirability of legislating in a particular policy area. Strategies of "pursuit and avoidance" occur when the disadvantaged party recognizes that failing to become involved in a high-profile legislative initiative will be harmful. This realization may lead them to introduce legislation that approximates, meets, or exceeds the legislation offered by the opposition. Pursuit strategies can be hazardous for the disadvantaged party, however, in that they commit themselves to a very different position than previously established, resulting in actual legislation or a bidding war with the opposition. Either way the distinction between the two parties becomes more modulated, and the disadvantaged party risks criticism from constituent groups. From the point of view of the advantaged party, compromise most likely means settling for less than they would like to receive, thus losing an important political advantage over the disadvantaged party.

Strategies of "encroachment" are most likely when there is no ground for agreement between the parties, yet the disadvantaged party perceives great risk in failing to address the policy issue. In an attempt to appear closer to the opposition than is actually the case, the disadvantaged party will offer a legislative alternative that obfuscates the differences between the two parties. In so doing, the disadvantaged party seeks to split the support for policy change, drawing away the backing of more moderate supporters of change and leaving no single policy coalition with a majority. This attempt will fail if the majority party is highly unified or if the disadvantaged party miscalculates the willingness of the majority party to accept its offer.

Gilmour then turns to the strategy of provoking a presidential veto. Setting aside vetoes that are the result of miscommunication, miscalculation, or misunderstanding, he argues that provoking a veto is a strategy that is unique to divided government and serves to distinguish the two parties from each other, meanwhile gaining support from constituent groups. This strategy is not be pursued consistently by the majority in Congress because of the tremendous costs in time and effort of passing a bill that will not be enacted; it is one that the majority will select with great care. It is also possible that the president, in an unanticipated move, will choose to sign the bill, blurring the differences between the parties and accepting partial credit for the policy change.

Gilmour concludes his discussion of strategy by examining the role of summit negotiations. Such negotiations are likely when neither political party has the resources to pass crucial legislation on its own and both face vociferous criticism from constituent groups for compromising. The value of summit negotiations is that they allow the two sides to negotiate in private, thus diffusing responsibility for the end product of the negotiations.

The final chapter, "Advice for Moral Politicians," seeks to provide counsel to politicians, suggesting that they should be more willing to compromise, thus benefiting their constituents. Gilmour argues that by compromising, opponents of the status quo make it more likely that broader reforms will be enacted in the future as ideological barriers to the expansion of programs are eliminated by a program that is proven to work. Yet, of all the chapters in this book, this one is the least convincing. The author spends a significant amount of time admonishing the public for not adequately monitoring their elected representatives, a surprising criticism given that the roots of this work are clearly planted in the soil of rational choice. Furthermore, I suspect that activists will be unconvinced by Gilmour's advice to compromise.

Readers will not find a discussion regarding the distinction between *individual* and *group* preferences. Gilmour assumes competition between two monolithic political parties without regard for individual differences within the political parties, an assumption with which many political scientists would be uncomfortable. As a theoretical exercise, beginning with this simple assumption, Gilmour provides a theoretical platform that will ultimately lead him or others to develop a more elaborate theory that incorporates both individual *and* group preferences. As such, it is a useful first step in a theory-building process.

Readers interested in analysis of speeches, floor votes, or interbranch conflict will not find it here; Gilmour does not engage in the analysis of any data aimed at confirming the many hypotheses that arise from his theoretical endeavors. The author may have plans for future empirical research that might include such analysis. If not, political scientists and graduate students have access to a theory that is rich with hypotheses waiting to be verified. Qualitative analysis of floor speeches and presidential addresses, floor votes, or presidential veto episodes all provide opportunities to explore the important empirical questions that Gilmour raises.

Others readers may be disturbed by the ideographic nature of the evidence Gilmour presents to support his arguments. Examples are sprinkled throughout chapters in a way that is often less than systematic and that may leave many readers unconvinced. Those interested in case studies that systematically examine political strategy, producing new interpretations of important social policies, will be disappointed. The case studies that are included here do not break new ground, nor do they *conclusively* support the author's argument. They are sufficiently illustrative, however, to allow the reader to understand how this framework might be applied to an empirical case.

It is important to note that the three previous criticisms were also leveled at important works in American politics, such as Mayhew's (1974) *Congress: The Electoral Connection* and Fiorina's (1977) *Congress: Key to the Washington Establishment*. However, each of these works spawned a generation of important scholarship on Congress in addition to reasoned criticism based on some of the same concerns expressed above. Like these authors and others, Gilmour has developed an important theoretical framework that deserves the careful attention and research effort that these earlier works have received.

If this book can be said to have a major weakness, it would be the lack of attention to the role of party leaders in strategic disagreement. The author discusses party leadership only to detail the differences in the leadership styles of Speakers Wright and Foley (pp. 62–3). Party leadership within Congress is important, however, for encouraging party unity. Leaders pursue distinctive strategies aimed at producing such unity, including persuading the public to support or oppose a particular proposal; their success or failure to persuade the public will affect leaders' ability to build partisan coalitions within Congress. If only for this reason, a discussion of congressional leadership would have been a welcome addition to Gilmour's theoretical framework.

Strategic Disagreement is a work with potentially broad appeal. It will be at home in undergraduate and graduate courses on political institutions; political journalists will also find it fascinating, as will any individual with a general interest in politics. I would like to add one caveat, aimed primarily at those who choose to use this book in the classroom, but also at interested journalists. Apparently, much of the current cynicism about contemporary politics arises from characterizations of politicians as unusually calculating and seemingly without regard for the public welfare, perhaps "immoral" as Gilmour's final chapter suggests. I would like to request that educators especially spend some time discussing the "better nature" of politicians, the high-minded principles that lead many or most politicians to seek public office, an impulse that from a cost-benefit perspective is irrational. For many politicians, failure to compromise reflects their conception of the common good rather than cynical positioning for the next election.

Autos, Smog, and Pollution Control: The Politics of Air Quality Management in California. By Wyn Grant. Aldershot, UK, and Brookfield, VT: Edward Elgar, 1995. 198p. $71.95.

John J. Kirlin, *University of Southern California*

A useful, comprehensive primer on development and implementation of air quality management policies in California, *Autos, Smog and Pollution Control* ranges widely. National, state, and local policies are examined, and considerable attention is given to efforts to develop mass transit alternatives to auto travel, especially rail-based systems. Professor Grant, Chair of the Department of Politics and International Studies at the University of Warwick (UK), brings the perspective of one experienced in centralized politics to California's complex, fragmented political system that is characterized also by frequent use of initiatives.

The Los Angeles basin, home to 15 million people, is famous for having the nation's most severe air quality problems. The city of Los Angeles initiated its efforts to improve air quality in 1944. In 1947 the geographically more encompassing county of Los Angeles took on this responsibility and 1976 state legislation created the South Coast Air Quality Management District, covering four counties overlaying the air basin. California's first state agency focusing on air quality was created in 1955 and the California Air Resources Board was created in 1967. The first national Clean Air Act was enacted in 1970, followed by the 1990 Clear Air Act. Because of California's prior history of local and state policy leadership and extreme problems, national air quality policies have afforded the state the opportunity to develop stricter state-specific policies, most recently undertaken in the California Clean Air Act of 1988, a primary feature of which is mandating the manufacture and sale of ultra low and zero-emission vehicles in California. Other national and state laws, especially those focused on transportation, also sought to improve air quality. California thus provides an excellent location within which to analyze the variety of policy approaches to improving air quality.

One strength of Professor Grant's volume is its ambitious breadth. Policies intended to improve air quality interact with other policies, are very expensive, can depress or stimulate state and regional economies, test capacity to overcome the political and agency fragmentation characteristic of our nation, and seek to change living, working, and travel behaviors of great importance to individuals and firms. Any simple understanding of the dynamics involved is very likely to be wrong, and any single, simple policy initiative is likely to fail to be implemented and to be thwarted. This volume should find use in classes on policymaking, planning, urban politics, public administration, and environmental studies as a "reality test case" against which theories, policy instruments, and analysis tools could be tested.

Professor Grant relies primarily on a "policy communities" approach to analysis. Its strengths are organizing multiple interests and agendas involved in air quality policies into categories whose membership and goals persist over time. To overcome possible static biases in this approach, he also discusses both the advocacy coalition approach of Sabatier and Jenkins-Smith's (1993) *Policy Change and Learning: An Advocacy Coalition Approach* and the streams approach of Kingdon's (1984) *Agendas, Alternatives and Public Policies*. However, these approaches are not fully employed even though they could have proven useful, especially in analysis of the impact of events external to air quality policy and of efforts by interests to shift policy choice to favored arenas. For example, the recession in California in the early 1990s encouraged curbing of the powers of the South Coast Air Quality Management District, and automobile manufacturers, electric utilities, and environmentalists all seek to control arenas of policy choice.

Finally, this analysis serves to crystallize two major challenges to democracy embedded in environmental policy choices. The first is how to integrate or make trade-offs among different policy goals. The second is how to avoid having critical policies made by technical elites with limited political accountability whose actions can erode citizens' accessibility to democratic participation in shaping their lives. Professor Grant laments the absence of attention to global warming among the California policymakers he interviewed, observing that EU directives on air quality are forcing this issue in Europe (although compliance by member states varies, with Britain resisting policies that are not cost-effective or that are too constraining of its economy). He also finds the existence of direct democracy via the initiative in California to be troubling.

Why would we want to design policymaking institutions that make trade-offs among different goals difficult and that routinely reduce political accountability and reduce opportunities for citizenship? Yet these are exactly the dynamics underlying most policymaking regarding environmental and species protection polices. Even achieving environmental goals is threatened by current approaches, as has effectively been argued in the recent analysis of the EPA by the National Academy of Public Administration's (1995) *Setting Priorities, Getting Results: A New Direction of EPA*. Yankelovich's (1991) *Coming to Public Judgement* advances a convincing argument for the centrality of citizens in effective democracy. This volume is very useful for understanding air quality policymaking; it is much less useful in suggesting how the current system of such policymaking could be improved.

Catastrophic Politics: The Rise and Fall of the Medicare Catastrophic Coverage Act of 1988. By Richard Himelfarb. University Park: Pennsylvania State University Press, 1995. 132p. $28.50 cloth, $13.95 paper.

Christine L. Day, *University of New Orleans*

The Medicare Catastrophic Coverage Act (MCCA), passed by overwhelming majorities in both chambers of Congress in 1988, was the largest expansion of Medicare since the program's inception. Funded entirely by beneficiaries themselves through a progressive financing scheme, the MCCA marked a major departure from previous social insurance policymaking. In a remarkable reversal, Congress voted, again overwhelmingly, to repeal the legislation just 18 months after it passed. Richard Himelfarb's fascinating account attempts to solve two puzzles: How could such an overtly redistributive policy have passed in the first place? And even more important and puzzling, what led to the shattering of consensus, the surging opposition, and the eventual repeal of the legislation?

Himelfarb's explanation for the fall of the MCCA contradicts two other explanations advanced by MCCA advocates and often supported in media accounts. One explanation suggests that the MCCA failed to gain widespread support because it did not cover long-term care, the catastrophic expense of greatest concern to most older people. However, Himelfarb notes that advocates of governmental long-term care coverage supported the MCCA as a foundation for further expansion in that area, and his analysis of public opinion data indicates that beneficiaries were much more concerned about the program's financing than about the lack of long-term care coverage. The second explanation for the surging opposition cited by MCCA advocates is the misleading and inflammatory campaign for repeal of the MCCA conducted by several interest groups, especially the National Committee to Preserve Social Security and Medicare. However, Himelfarb contends that widespread opposition would have arisen even without the National Committee's often misleading campaign against the legislation.

Himelfarb's own explanation for the fall of the MCCA is complex, but focuses primarily on the failure on the part of the MCCA's architects and advocates "to address clearly and honestly the redistributive implications of the legislation's financing" (p. 80). Why did policymakers support such a politically treacherous, beneficiaries-only, income-related financing scheme in the first place? In an era of fiscal scarcity, the MCCA's architects had sought to fund the expanded benefits without unduly burdening low-income seniors or increasing taxes for the general population. The precedent for income-related financing of social insurance programs had been set previously—for example, in the Social Security Reform Act of 1983—without any great political fallout. The MCCA's financing mechanism represented an even greater departure from previous social insurance policy, by taxing only current beneficiaries in an overtly redistributive fashion; about 30% of beneficiaries would actually be worse off under the program. This method of financing had won the support in Congress of both liberals seeking to expand federal benefits during an era of scarce resources and conservatives seeking to reduce the federal deficit.

Policies that involve widespread benefits and concentrated costs, however, are difficult to sell to the public. They generate intense opposition from the minority of people who feel they have something to lose, while the majority that stands to gain tends to be less fervent and more diffuse in its support. Thus, social insurance advocates have long sought to obscure the redistributive aspects of such programs as Social Security and to perpetuate the public perception that benefits are "earned" through contributions to the program. In the same way, MCCA advocates sought to emphasize the program's benefits and downplay the costs, especially the costs to higher-income seniors. A belated effort to explain and defend MCCA's redistributive implications only served to increase opposition to the program, as beneficiaries at all income levels felt they had previously been deceived.

Of particular interest here is Himelfarb's analysis of the precipitous decline in support for the MCCA after its passage. The analysis makes use of elderly opinion data collected for the American Association of Retired Persons, with detailed questions about respondents' knowledge, attitudes, and expectations regarding various specific provisions of the MCCA. The author notes that surveys conducted prior to the MCCA's adoption, while sufficiently favorable to encourage passage, did not adequately address the trade-offs between costs and benefits, and thus inflated the degree of support. Still, many of the findings about the nature of the opposition to the MCCA are perplexing. At least 60% of elderly beneficiaries, especially those with relatively low incomes, stood to receive benefits in excess of costs. Yet respondents with the highest degrees of knowledge and familiarity with the MCCA were most likely to oppose the program, and this held true across all income levels.

Himelfarb hypothesizes—and his analysis supports the hypothesis—that survey questions measuring knowledge about the MCCA were actually gauging positive or negative attitudes toward the program. Thus, for example, lower-income respondents who professed knowledge about the surtax, which did not affect them, may have simply been projecting negative attributes upon a program they already had decided they disliked. For some reason, the author buries this interesting finding in a methodological appendix, perhaps because he is targeting a wider audience than social scientists familiar with techniques of multivariate analysis. But it would have been better incorporated into the public opinion discussion in chapter 5, both because it is interesting and relevant to that discussion and because Himelfarb's presentation is sufficiently clear and engaging for nonspecialists.

Catastrophic Politics offers a comprehensive analysis of the rise and fall of the MCCA using a wealth of primary and journalistic source materials. Much can be learned from the book, not only about this program but about general social insurance policymaking and entitlement reform in an era of fiscal retrenchment and structural budget deficits. The dilemma of how to distribute the costs of social insurance programs in the face of deficit-reduction pressures remains to be solved, however. Himelfarb reviews two opposing perspectives without trying to predict which is most likely to prevail in political debates (a wise choice given the volatile nature of social insurance politics these days). One perspective, offered by fiscal conservatives, argues that funding must be increasingly income-related, without hiding the redistributive nature of such approaches. The other perspective, held by social insurance and elderly advocates, contends that program financing must continue to be broad-based and intergenerational, or else social insurance programs will lose their base of public support. In fact, academics such as Jill Quadagno (who is not cited in the book, but should have been) argue that this loss of support—and the subsequent privatization of social insurance—is the actual goal of many fiscal conservatives.

Overall, *Catastrophic Politics* is an important contribution to the literature on social insurance policy, meriting attention outside of, as well as within, the scholarly community.

Poverty and Power: The Political Representation of Poor Americans. By Douglas Imig. Lincoln: University of Nebraska Press, 1996. 159p. $37.50.

Kay Lehman Schlozman, *Boston College*

Douglas Imig's investigation of the response of advocates for the poor to the political changes wrought by the Reagan administration is the best kind of case study. On the one hand, it is theoretically informed, engaging intellectual issues that have long absorbed the attention of students of both citizen participation and organized interests and never losing sight of the "in what" part of the old question, "A case study in what?" On the other hand, the substance is itself both interesting and important. It matters for policy outcomes, and for the well-being of millions of citizens, what and how much the government hears from the poor themselves or from advocates on their behalf.

The empirical heart of the enterprise is a longitudinal investigation over the course of the decade of the 1980s of both organizational and protest activity in opposition to Reagan administration cuts in assistance to the needy. With respect to organizational activity, Imig uses a variety of sources of information including annual reports to consider how six organizations that focus on hunger issues—three of which were dependent upon federal largesse for organizational survival and three of which were not—confronted the threats posed by the Reagan administration: programmatic cutbacks in social services and reductions in funding for poverty advocacy. Not unexpectedly, the three organizations that had not been nurtured financially by the government fared better, and all six organizations were forced into a position of special sensitivity to the preferences of the foundations and philanthropists that foot the bill for nonprofits. Beyond that, however, what is fascinating is the enormous variation among the organizations with respect to the way in which the dictates of organizational maintenance affected the commitment to either political advocacy or direct provision of services. Each of the six followed a slightly different pattern in dealing with the changed political environment.

Imig's analysis of protest activity concentrates on the issue of homelessness, showing how protests involving mild civil disobedience early in the decade later gave way to more mainstream forms of protest and how the volume of protest increased in congressional election years. He documents more such activity than I had remembered—or perhaps, even, than I had known about at the time. In this context, comparative data might have been helpful. I would surmise that protest focused on poverty issues was dwarfed at the national level by protest over abortion and at the local level by protest about education. Still, Imig ultimately concludes, to his obvious regret, that protest activity over poverty issues during the 1980s cannot be considered a social movement.

Imig argues that the protests during the Reagan years were essentially reactive, arising in response to cuts in federal programs. He also demonstrates a growing symbolic response to poverty issues in the form of increased numbers of congressional hearings on the subject—although whether this outcome was generated by the protest activity, by enhanced media attentiveness to the subject, or by some other factor is not clear. The symbolic response was not matched by a tangible response; nevertheless, had there been no mobilization on behalf of the poor, cuts in social spending might have been even more severe.

In many ways, Imig's findings lend credence to several approaches to understanding joint participation in politics. It is possible to interpret his analysis as providing confirmation of the insight of pluralism that political action arises in response to changes in the political environment, the insight of collective action theory that joint activity by large publics is not inevitable, and the insight of the political process model of social movements that political opportunity structures shape the possibilities for collective mobilization. Yet, the implicit message in Imig's analysis is that there are limits to the utility of generalized approaches to collective action and political advocacy.

When it comes to political representation, the poor face special difficulties. The free rider problem that, presumably, confronts any large group has not proven to be an insurmountable obstacle to veterans or gun enthusiasts. What differentiates these groups from the poor is not their size or the intensity of their stake in government action, but their political resources. On the supposedly level playing field of politics in a democracy, those who lack traditional political resources operate at a handicap. If they are represented at all, the disadvantaged invariably depend, at least in part, on the efforts of advocates on their behalf. Inevitably, these surrogates have concerns and priorities of their own. During the 1980s the traditional allies of the poor—for example, unions and civil rights groups—were preoccupied with other concerns, including their own organizational weakness. Imig makes clear that the poor face an additional impediment in politics. In contrast to other resource-deprived groups, such as the developmentally disabled, the poor cannot rely on the automatic good will and sympathy of the American people. Drawing on a strain running deep in the American ideology, many Americans consider the poor to be responsible for their own circumstances. As a consequence, advocates for the poor are forced to redefine issues of poverty in order to focus on more obviously "deserving" groups like the hungry or children.

The scriptures tell us that the poor will always be with us. This brief, but illuminating volume makes clear that in all likelihood they will rarely be with us as political activists.

Public Attitudes toward Church and State. By Ted G. Jelen and Clyde Wilcox. Armonk, NY: M.E. Sharpe, 1995. $50.00 cloth, $19.95 paper.

J. Christopher Soper, *Pepperdine University*

Public Attitudes toward Church and State provides a thoughtful and effective analysis of mass and elite views of religious free-exercise and establishment clause issues. The book makes outstanding use of the 1987 Williamsburg survey, which, despite the limitations that the authors correctly note, remains the most comprehensive national survey of public opinion on church-state matters ever conducted. Jelen and Wilcox supplement the Williamsburg survey with two data sources of their own: a survey of residents of the Washington, D.C., area and qualitative focus-group interviews with respondents from various religious traditions. What is most impressive about this book is that it combines an analytically sophisticated description of group attitudes on church-state issues with a theory to explain how those opinions form and are maintained.

The authors' treatment of mass and elite attitudes toward establishment clause issues is the most interesting and informative material in the book. Jelen and Wilcox demonstrate a strong correlation between religiosity and opinion on issues such as government funds for religious schools, a moment of silence in public schools, the use of school rooms for religious meetings, and the teaching of evolution and creationism in public schools, all matters adjudicated by the Supreme Court in recent years. Support for a high wall of separation between church and state and no aid to religion are most common among mainline Protestants, Jews, and respondents with no religious preference. Roman Catholics and evangelicals are most likely to be pure accommodationists.

While this is a not surprising result, the authors conclude—contrary to what the data at first glance seem to indicate—that the surveys do not support the popular culture wars thesis. The authors effectively use multivariate analysis to show that opinions on church-state issues are more varied and complex than is commonly assumed by advocates of a culture wars thesis, there is no uniform elite attitude on these issues, and there is little consensus among strict separationists and accommodationists on why they adopt their positions. Jelen and Wilcox discovered an identical complexity on public attitudes toward religious free-exercise issues. This observation has important political ramifications, particularly for recent efforts by the Christian Right to form a political coalition between evangelicals and Catholics on religious issues. Such a coalition is possible on some, but not all, religious policy issues; and even where there is consensus, different principles underlie similar views and mitigate against the simple dichotomy of religionists against nonreligionists that is popular among politicians and pundits alike.

The authors also discern an apparent inconsistency in the public's attitude on establishment clause issues. In the abstract, the public supports the idea of church-state separation; but when it comes to the concrete application of this idea on particular policy issues, the public generally favors an accommodation between church and state. Significantly, elite attitudes are no more consistent than those of nonelite respondents. Jelen and Wilcox contend that these apparent inconsistencies mask a public that actually structures its beliefs on church-state issues in a meaningful fashion. This is a bold assertion that contradicts the easy conclusions that there is no connection between abstract principles and concrete application, because the public has not thought much about these issues, or that the public is hopelessly confused and hypocritical in its application of these concepts.

Jelen and Wilcox theorize that support for accommodation is high because the public does not imagine that the issues in question might arouse controversy and because most Americans regard accommodation as an acceptable way to further religion's role as a source of social cohesion and consensus. Moreover, the authors use the responses to establishment and free-exercise clause issues to present a typology of four possible church-state positions: religious nonpreferentialists, Christian preferentialists, religious minimalists, and religious free-marketeers. They show that each position is empirically and conceptually distinct, represented by different sets of elites in the general public, and provide adherents with a theory to shape their views on concrete church-state issues.

Public Attitudes toward Church and State does an outstanding job analyzing the survey data and raising important questions related to church-state issues. There are several occasions, however, when Jelen and Wilcox might fruitfully have elaborated on some of the implications of their data. For example, the authors note a divergence between public opinion and Supreme Court jurisdiction on various establishment clause issues but do not provide much discussion about what significance, if any, this difference portends. Does this finding undermine the argument that Supreme Court decisions, even unpopular ones, can be an agent for changing public opinion over time?

Moreover, while I sympathize with the claim that the public has more sophisticated views on church-state issues than is commonly assumed, it is hard to shake the impression that self-interest is as reliable a predictor of public attitudes toward church-state issues as religious ideology. It cannot be entirely accidental that religious free-marketeers are disproportionately Jewish and secular, evangelical and other orthodox Protestants fill the ranks of Christian preferentialists, and religious nonpreferentialists are represented by those churches that have been the historical victims of discrimination. This raises the larger issues of the nature of an ideology, its relationship to a group's self-interest, and its relative autonomy from other social and cultural variables. These minor points aside, *Public Attitudes toward Church and State* is a valuable book for students and scholars of religion and politics, survey research, and church-state issues.

Who Controls Public Lands?: Mining, Forestry and Grazing Policies, 1870–1990. By Christopher McGrory Klyza. Chapel Hill: University of North Carolina Press, 1996. 211p. $34.95 cloth, $14.95 paper.

Robert H. Nelson, *University of Maryland*

Who Controls Public Lands? argues that four basic ideas have dominated policies for the 30% of the land area of the United States now owned by the federal government. During the nineteenth century, "economic liberalism" looked to the market as an appropriate benchmark and saw government as having a minimal role beyond disposing of lands to private owners. In the Progressive Era, "technocratic utilitarianism" emerged as a strong challenger, acting to retain the lands in public ownership and to supplant the market with management decisions made by professional experts in government. "Preservationism," which also emerged as a major influence early in this century, saw nature in spiritual rather than scientific terms and aimed to set lands aside in areas where human impact would be limited. Finally, a more recent political philosophy, "interest-group liberalism," regards public land actions as legitimate when they serve the goals of the affected interests.

According to Klyza, one or another of these ideas has tended to dominate different areas of public land policy over very long periods. Thus, the mining law was shaped in the mid-nineteenth century according to the tenets of economic liberalism; despite many efforts to change it, the law still survives in close to its original form. The Forest Service was formed in 1905 around an ethos of technocratic utilitarianism and still sees itself in these terms. The management of grazing lands by the Bureau of Land Management exemplifies the dominance of interest-group forces —in this case private livestock operators—as formally codified by the Taylor Grazing Act of 1934 and not greatly changed to the present. In Klyza's overall scheme preservationism plays the role of challenger to the reigning thinking in each of these areas. (The book says little about the National Park Service, formed in 1916, for whom preservationism has in fact been the dominant ethos.)

Klyza develops a series of case studies of legislative and executive controversies that are used to illustrate the historical interplay of these ideas. He relates the long story ending with the enactment in 1964 of the Wilderness Act, which revolved in part around a clash between the traditional rights of access of miners to public lands and the preservationist objective to exclude mining. In the early 1980s, as Klyza relates, the failed attempt by the Reagan administration to privatize portions of public lands pitted latter-day economic liberals against both technocratic utilitarians and preservationists.

To be sure, sale of lands also threatened the "extralegal property right systems" (p. 145) under which existing public land users have long maintained access to public lands. A successful privatization campaign might have sought to recognize and formally confirm the de facto rights of existing users. However, the Reagan administration made the mistake of trying in effect to abolish these informal rights through outright sales of the land.

Klyza concludes that all of the ideas of the past are in one way or another inadequate to guide public land management in the future. Lacking any clear sense of direction, the current interaction of conflicting intellectual and political forces has yielded a "gridlock of pluralist politics" (p. 159) that persists only because "once an institution is firmly established and a privileged idea embedded within it, that institution is very difficult to change" (p. 158). Indeed, unless there is "fundamental change, it is unlikely that our institutions will be sufficient to help us through the environmental crisis" (p. 160) that Klyza sees confronting the world today. The book nevertheless leaves it to others to do the necessary exploration of alternative designs for a future public land system.

There is much that is reasonable and useful in *Who Controls Public Lands?* To be sure, many others have said similar things, sometimes more persuasively. The book is repetitive, partly because Klyza sometimes lets his organizing scheme get in the way of telling a good story. The book makes no claim to develop new data or to probe previously unexplored archival or other materials.

Klyza regards his real contribution as the analysis of the role of ideas in the public land process. However, in some cases his intellectual categories serve as much to confuse as to clarify the issues. For example, the characterization of the management of public grazing lands since the 1930s as an example of interest-group liberalism is strained. With good reason many others have seen the situation in just the opposite terms—the domination of one group, the ranchers, to the exclusion of others. Klyza's portrayal of economic liberalism also misses important elements. Some of the strongest advocates of reliance on market methods on public lands have been economists in the Office of Management and Budget and other federal agencies. Yet these career professionals are not economic liberals; rather, they are closest in basic attitude to the technocratic utilitarians—

they simply see the market as a particularly effective tool of government management.

At the same time, Klyza's seeming defenders of economic liberalism—the miners, livestock grazers, and other commodity producers—have often in practice been hostile to market forces. They have looked to the federal government as a friendly dispenser of subsidies and as a way of protecting them from competition. Cattle ranchers, for example, were among the leading defenders of the Forest Service early in this century, because it would keep out homesteading on national forest grazing areas and curb the activities of migratory sheepherders.

As Klyza interprets matters, the survival of the mining law can be attributed to the long-lingering influence of an outdated pro-market set of ideas. However, there are many economic considerations that even today make it difficult for a technical utilitarian to find a satisfactory compromise that is fair in terms of revenue and other government goals while also maintaining an adequate incentive for private industry to explore for new minerals. In leaving out this kind of economic detail, the book neglects some of the key factors that have influenced the long-running debate over the mining law. Important economic and administrative considerations also receive short shrift in other discussions of public land issues and history.

All in all, although *Who Controls Public Lands?* develops some interesting material and makes some thoughtful observations, specialists in public land policy will not find much that is new. The book may be helpful to those seeking a general overview of the subject, but more systematic treatments of public land history are available elsewhere.

Young v. Old: Generational Combat in the 21st Century. By Susan A. MacManus with Patricia A. Turner. Boulder, CO: Westview, 1996. 302p. $65.00 cloth, $21.95 paper.

William G. Mayer, *Northeastern University*

The age profile of the American population, it has long been recognized, has changed substantially over the last several decades. The birth rate has been declining, life expectancy is increasing, the pace and sources of immigration have shifted. As a result, the elderly account for a significantly larger percentage of the population; children and young adults are proportionately less numerous. And all indications are that such trends will continue—possibly even accelerate—until well into the next century.

These developments have attracted a good deal of attention from economists, demographers, and sociologists—but surprisingly little from political scientists. We know a fair amount, for example, about what the changing age distribution will do to the Social Security and health care systems in the years ahead, but much less about what it will do to our politics and about how that altered political context will shape and constrain the search for new policies.

Fortunately, Susan A. MacManus tries to fill that gap with her book *Young v. Old*, a comprehensive examination of generational and age-related differences in political attitudes and behaviors. One book will not, of course, answer all the questions that political scientists might reasonably pose about this important set of issues. But MacManus has provided a very good starting point: a useful and informative book about one of the major challenges facing the United States in the twenty-first century.

After an introductory overview of the nation's changing age profile, most of this book consists of an extensive examination of whether and how various age groups differ along two major dimensions: political participation and public policy preferences. In terms of political participation, much of MacManus's analysis is not particularly new. She finds, as have a host of previous studies, that the young are less likely to register and vote than their elders and that they are less inclined to identify with a political party. MacManus also claims that the participation gap between young and old is likely to narrow in the future, but the only solid evidence for this conclusion is the small increase in youth voting that occurred in 1992. Of considerably more value is the author's detailed examination of nonelectoral forms of participation (e.g., contacting public officials, joining groups, running for office, and so forth).

Perhaps the most thought-provoking section in the entire book is the analysis in chapter 5 of age-related differences in attitudes about taxing and spending. Standard survey questions on these topics, especially those that merely ask whether the respondent would like more, less, or the same amount of money spent on a given problem area, generally show rather modest differences between young and old Americans. But this apparent harmony dissolves, MacManus persuasively argues, when researchers probe more deeply into priorities and trade-offs. For example, while all age groups express general approbation for a national health insurance system, the elderly are most interested in making long-term care more affordable and in reducing paperwork, while younger cohorts are more concerned with providing health insurance for those who currently cannot afford it.

Though most of this analysis is well executed and clearly presented, I was less pleased with MacManus's efforts to determine if the age-related differences that do exist are due to the *generation* a set of respondents belonged to or to their position in the *life cycle*. This issue is, to say the least, an exceedingly difficult one to resolve. As has often been noted, one cannot control for generation, age, and time period at the same time; an appeal must be made to "side information." Yet MacManus seems largely unaware of these complexities, and frequently renders judgments that seem inadequately supported.

To take just one example: When MacManus's data show 18- to 29-year-olds more supportive of most forms of U.S. military intervention in foreign countries, she quickly concludes that the pattern has a generational explanation. "A person's outlook on the appropriate U.S. role in foreign affairs also greatly depends on the outcome of U.S. involvement in a war, confrontation, or other crisis that occurred at the time he or she was coming of age politically. If the outcome was judged successful, that person tends to be more positive toward future U.S. involvement (the Reagan generation and its experience with the Persian Gulf War and Somalia). If not, he or she is more negative (the Civil Rights/Vietnam/Watergate generation)" (p. 224). But this ignores the well-known finding that through most of the Vietnam era, young people as a whole were actually *more* supportive of the war than their elders. Moreover, MacManus's data (Table 6.8, p. 233) show that the age group most opposed to military intervention was not the Vietnam generation but the elderly, whose war experiences, especially with regard to World War II, would seem to be considerably more favorable than anything that has happened recently in Somalia or the Persian Gulf.

Such caveats notwithstanding, there is, I think, no gainsaying this book's central premises: that the age distribution of the American population has and will continue to change, and that many significant policy controversies are likely to polarize the citizenry along age-related lines.

MacManus's recognition of these realities—and the substantial wealth of evidence and insight she brings to bear on them—make this book a definite contribution to the political science literature.

With Malice toward Some: How People Make Civil Liberties Judgments. By George E. Marcus, John L. Sullivan, Elizabeth Theiss-Morse, and Sandra L. Wood. New York: Cambridge University Press, 1995. 288p. $59.95 cloth, $19.95 paper.

James L. Gibson, *University of Houston*

When I finished reading *Political Tolerance in American Democracy* by John Sullivan, Jim Piereson, and George Marcus (1982) many years ago, I thought it the definitive work on political tolerance. In addition to providing a thorough theoretical justification for studying mass political intolerance, the book laid out a new conceptualization of tolerance, developed some unusual measures, and presented an uncommonly comprehensive analysis of the origins of intolerance. The models and methods that emerged from that book have dominated the field for well over a decade.

With so many aspects of political intolerance so well understood, one would expect little that is revolutionary to be published these days. Fortunately for the discipline, these expectations are wrong, for Marcus, Sullivan, Theiss-Morse, and Wood have produced a new blockbuster on political tolerance. Just as the field was shown a new way of thinking about tolerance by the original book, *With Malice toward Some* gives us an entirely innovative approach to understanding the dynamics of political intolerance.

The key insight of this new book is that intolerance is not a fixed attribute of individuals, but instead people make judgments about whether to tolerate based on their predispositions, previously established beliefs, and current information about the political context. The theoretical framework, loosely combining symbolic politics with rational choice, therefore focuses on the interplay of values and personality attributes acquired early in life with information and calculus associated with contemporaneous events. Nor are they impervious to the role of individual differences in this process, using a formulation not unlike that proposed by McGuire many years ago to account for the degree to which people are responsive to information emanating in their environments. Thus, this book is part of an important trend in public opinion research toward viewing opinions as highly dynamic and contextual. The dependent variable in their research is not the degree of intolerance—the conventional concern of most tolerance research—but is instead *how people make tolerance judgments*. This is an important difference, one that distinguishes this book from most of the earlier work in the field.

A second very important contribution of this book is its concern with the origins of threat perceptions, especially with the role of emotion—in contrast to reason—in processing threatening stimuli. One of the most significant unanswered questions in the tolerance literature is why some people are threatened by their political enemies while others are not. Relying on the work of the psychologist Jeffrey Gray, Marcus, Sullivan, Theiss-Morse, and Wood develop an intriguing theoretical model of the mechanisms by which external stimuli generate internal anxiety, which is then understood and articulated as threat. For instance, one interesting hypothesis is that anxiety enhances the role of contemporaneous stimuli in tolerance judgments, thereby diminishing the influence of predispositions. Although they do not solve the riddle of the origins of generalized propensities to perceive threats from one's political enemies, this portion of their analysis generates many important clues as to how threat gets transformed into intolerance (e.g., perhaps surprisingly, this has little if anything to do with the potential political power of the disliked group).

Hoping to reject Stouffer's original proposition that tolerance requires a "sober second thought"—nonemotional deliberation—Marcus, Sullivan, Theiss-Morse, and Wood also hypothesize that emotional decision making may serve tolerance *or* intolerance, depending on the context. Despite an obvious preference for a contrary finding, their analysis confirms the view that the injection of emotion into civil liberties disputes is *more likely* to produce an intolerant, rather than a tolerant, outcome. Indeed, this is one of the strongest and most important findings of their analysis.

As one who has probably read the work of this research team more closely and carefully than virtually anyone, I would be remiss not to raise some issues that will surely be debated by those who scrutinize the book in some detail. For instance, my overall impression is that the authors strain very hard to support Gray's personality theory. Many of their findings are compatible with theories having nothing to do with Gray's understanding of personality, even if they are not incompatible with his theory. On occasion the analysis seems to decidedly *not to support the theory*, even though this is not clearly acknowledged in the text. Those who want to ignore entirely the theoretical apparatus imported from Gray will find it easy and perhaps fruitful to do so.

Indeed, there are several points in this book at which the data are not very cooperative with the theory or hypothesis. On occasion, the authors claim support for hypotheses from extremely weak coefficients (should an unstandardized coefficient of −.09, significant at .09 with 589 cases be taken as support for a hypothesis, as it is on pp. 110–1?) As one who is all too accustomed to working with unruly and irascible survey data, my complaint is not that of a purist. But the discerning reader will surely derive fewer supported hypotheses from these analyses than do the authors.

One of the most disappointing sections of the book deals with the role of personality. Trying hard to demonstrate that Gray's theory of personality has an impact on tolerance, they first encounter difficulties measuring personality attributes. Then in one study, one dimension of personality has an enormous effect on tolerance judges (b = .51, p. 169), but in the next study its effect is weak at best (b = .16, p. 171). Unfortunately, there is little speculation as to the reason for this inconsistency. And surprisingly, the impact of personality on threat perceptions is not investigated; indeed, even the direct effect of personality on general tolerance attitudes is implied rather than tested (although their earlier work has clearly established this relationship). Their conclusion that personality plays an important role in tolerance is surely correct—and their speculations about how personality attributes affect attentiveness to environmental stimuli is provocative, even if only weakly supported. But the exact nature of the connection between personality and intolerance still provides some important riddles.

Some readers may object to the use of college students as subjects for most of the experiments, and there are some instances in which this seems an entirely reasonable concern. For instance, university students were presented with arguments either favoring or opposing political tolerance. Those receiving arguments favoring tolerance seemed to be more successful in arriving at a tolerance judgment in a hypothetical civil liberties dispute, but only to the slightest (and perhaps trivial) degree. One would surely expect the impact of persuasive arguments to be greater within a less sophisticated sample. In fairness, Marcus, Sullivan, Theiss-Morse, and Wood are not at all insensitive to this sort of concern about their samples, and they validated some of their experiments on subjects who were not college students. But there can be little doubt that future work will need to move further outside the college classroom.

In the final analysis, what makes this book special is not that it is the definitive word on tolerance judgments. Much of the analysis is inconclusive, and there is little doubt that we will fight in the future about whether some of the theoretical claims of the book are supported by their data, or are confirmed by subsequent analyses. But just as Sullivan, Piereson, and Marcus did in *Political Tolerance in American Democracy*, Marcus, Sullivan, Theiss-Morse, and Wood have given the field something to focus on for the next decade or so. I confidently predict that this book will have at least some impact on virtually everyone in the field for the foreseeable future. Moreover, since this is ultimately a book about political psychology, I hope the book will be a vehicle through which those with little interest in political tolerance will find the theoretical advances being made among those who study political tolerance. None who care about public opinion and democracy can afford to ignore this most important book.

The Dilemma of the Fetus: Fetal Research, Medical Progress, and Moral Politics. By Steven Maynard-Moody. New York: St. Martin's Press, 1995. 235p. $23.95.

Patricia Boling, *Purdue University*

Steven Maynard-Moody has written a lucid, gripping, and fair history of the public debate over fetal research. The story begins in the years preceding *Roe v. Wade*, with dawning public dismay over unethical medical experimentation on human subjects in the Tuskegee Syphilis Study, the horror of fetal deformities caused by thalidomide, and unbounded faith in the ability of medical science to find treatments and cures for such ills.

Central to the debate over federal funding for fetal research was the emergence of a well-organized pro-life movement after *Roe v. Wade* was decided in 1973 and the rise of opposition to research that depended on a ready source of fetal tissue, the fetuses from legal abortions. Maynard-Moody also explores how the widespread use of sonography and diagnostic, medical, and surgical advances have transformed the fetus from an obscure entity contained within the woman's body to the "second patient," increasingly viewed as a person with interests and rights of its own that may conflict with the woman's.

Maynard-Moody explains the ebbs and flows of fetal research over the twenty years from the early 1970s to the early 1990s cogently and dramatically. *The Dilemma of the Fetus* is not a dull, policy-wonk read, but an even-handed analysis of the changing political fortunes of fetal research in full control of the passionate and acrimonious debates unleashed by this issue.

In the first and last chapters, which frame the story of political controversy over fetal tissue research, Maynard-Moody argues that controversy is central to democratic control over science policy, because it counters the tendency of citizens and public officials to feel so ignorant and incompetent to judge issues raised by scientific research that they leave the experts to decide. "Democratic control over science," he writes, "requires that we embrace conflicts, even seemingly interminable and pointless ones. We must, in short, place the value of politics over the promise of science. We can no longer dismiss political intrusion into scientific decisions as a threat to intellectual freedom but must see it as an essential element *in strong democracy*" (p. 189).

The author's view that political judgment must be given priority in conflicts between scientific progress and the human and ethical costs or dangers it poses holds strong appeal. But Maynard-Moody gives his readers confusing signals about how he weighs the seriousness of the objections raised by the anti-fetal research lobby: one feels at times that he dismisses their position as driven by pro-life ideology and is himself persuaded by the pro-research arguments. Yet he also argues that even irrational or nonnegotiable moral views about the status of the human fetus should be allowed to call into question and slow down scientific progress. The latter position raises important questions for policy analysis: can we—*should* we—distinguish among different arguments against scientific research? Should *all* objections to research agendas be taken seriously? Or all objections that garner significant political power, measured in terms of media attention, public visibility, and hearings and votes in committees and Congress? To put it more baldly, are "no nothing" objections to scientific research deserving of serious attention? Should they slow down public decision making about what scientific research to fund? Should some threshold—e.g., the existence of serious public discussions among bioethicists or the ability to express ethical dilemmas in terms that are not narrowly and strictly sectarian—have to be crossed before science policymakers feel obliged to respond to objections? Maynard-Moody does not try to answer such questions; he says he favors any argument that slows the policy decision process down and empowers citizens to raise questions about the difficult ethical and human dimensions of scientific research. Whatever the initial reason for taking a closer look at research protocols and agendas, Maynard-Moody seems confident such scrutiny will allow serious and valid objections to be articulated. Yet the case he makes for the value of stopping to deal with controversy is undercut by the doubts he raises about the validity of the anti-abortion camp's objections to fetal research.

A second problem with Maynard-Moody's argument for democratic control over scientific research is its willingness to take as axiomatic the value of popular control over policy issues that seem arcane and beyond the capacity of nonexperts to judge, relying on Ben Barber's notion of "strong democracy." The empowerment of ordinary citizens to question the values of the scientific research community and to raise objections, even ones that seem benighted or narrow-minded by the standards of that community, ought to be connected to a powerful and situated articulation of what is gained from broader participation in politics. One wants more than a tepid reliance on Barber's ideas here. If this is the crucial point Maynard-Moody wants to make in his book, he should give us a persuasive argument for why policy and citizens will benefit from encouraging objections to the dominant paradigm of the scientific community.

Finally, Maynard-Moody barely considers the broad stakes of his argument, which are important for thinking about a whole range of emerging ethical and political issues raised by scientific and technological progress. For example, how do the objections raised by the critics of fetal research help citizens think more broadly about the values implicit in scientific research? How might this debate help us understand related debates about such issues as the possibility that widespread use of sophisticated technologies for ensuring optimal birth outcomes will undermine women's right to bodily autonomy, or the danger that defining *life* in terms of brain wave activity will justify widespread euthanasia for those on life-support systems and consequent erosion of respect for human life?

Despite my criticisms, I found this book very interesting and important. The story of political debate about fetal research is one of genuine drama and pathos, and Maynard-Moody tells it well. This book speaks to crucial issues of democratic control of scientific research and the ability to make human sense of revolutionary scientific and technological advances. If Maynard-Moody does not always spell out the implications of his research for other science policy issues, his readers surely will.

Nothing to Read: Newspapers and Elections in a Social Experiment. By Jeffrey J. Mondak. Ann Arbor: University of Michigan Press. 1996. 191p. $39.50 cloth, $16.95 paper.

Marion R. Just, *Wellesley College*

When media researcher Jeff Mondak encountered an obstacle in the shape of the 1992 Pittsburgh newspaper strike, he imaginatively turned it into a research opportunity. The population of Pittsburgh became the control subjects in a natural experiment on the role of newspapers in elections. By pairing Pittsburgh and Cleveland—cities similar in size and demographic characteristics—Mondak's quasi-experiment successfully addresses a number of controversial questions. What is the distinctive contribution of newspapers, if any, to political discourse? Do newspapers provide more information to the electorate than other news media? Does the absence of newspapers depress political activity?

Relying on survey data, Mondak demonstrates that, in spite of the newspapers strike, the Pittsburgh electorate was no less informed about the presidential and Senate races than Cleveland voters. The unique informational contribution of newspapers was confined to contests for the House of Representatives. The newspaper-deprived Pittsburgh sample registered significantly lower subjective levels of information and political discussion than the experimental-control voters in Cleveland only in regard to congressional races.

There are some limitations to this well-executed study that arise from the methodology. Because the evidence is drawn almost exclusively from public opinion surveys, we can only surmise that newspapers do indeed provide important information about House contests. The logic of the argument seems unassailable. National media such as network television or leading newspapers rarely cover specific House races. Local TV news in large metropolitan areas is no better. Because an urban media market often covers several congressional districts, the news about any particular House race is of little interest to a large part of the audience. For the same reason, House candidates rarely buy advertising time in large television markets, so as not to waste money on media consumers outside their districts.

A multimethod research design involving content analysis of local media could have lent further support to Mondak's contention that newspapers are a uniquely useful source of information about certain political campaigns. It would have been helpful, for example, to see what the previous coverage of House contests was in the Pittsburgh newspapers, so we could estimate just what people were missing in 1992. While newspapers are the logical suppliers of information about elections for the House of Representatives, they may not always perform as we expect. Pittsburgh newspapers may have historically provided poor coverage of House races, and Pittsburgh voters may have always felt less well informed about those races than voters in Cleveland. The opposite may be true of Cleveland. Perhaps in that city the newspapers have traditionally covered House contests like a glove, and more so than in other cities of comparable size.

Although he slights evidence about actual press content, Mondak recognizes that there is another factor that may have affected the subjective information level of Pittsburgh voters. The data show that Pittsburgh's candidates for Congress raised less money than their counterparts in Cleveland. Mondak points out that because of the newspaper strike, Pittsburgh House candidates had a more difficult time reaching out to potential contributors and consequently had even less money to spend on advertising than they otherwise would have.

The significance of candidate advertising is highlighted by what Mondak found in the case of the Senate races. Voters in Pittsburgh and Cleveland made comparable assessments of their access to information about the Senate contests, even though the Allegheny County citizens were denied newspaper coverage. In fact, the Pittsburgh sample registered a somewhat greater level of discussion of the Senate contest than the Cleveland sample. Mondak chalks up the difference to the greater coverage that the Specter-Yeakel race had in national media outlets, but he might also give credit to the Pennsylvania advertising campaign. Discussion among Pittsburgh voters might indeed have been stimulated by the well-funded Specter ad campaign.

Mondak's careful presentation of results for the presidential contest should be especially commended, given the natural reluctance of social scientists to illustrate null findings. Mondak recognizes that he has an important contribution to make to the debate about the value of newspapers. His study shows that those who, out of choice or necessity, rely on television rather than newspapers are no worse off in objective tests and feel no less inadequate in subjective assessments of their information level than those who have access to newspapers. His analysis strengthens the hand of those such as Graber, Neuman, Just, Crigler, Price, and Zaller, who find that differences in information level between people who are newspaper- or television-reliant result from the personal characteristics of the individuals and not the nature of the medium.

Mondak might be generous in his assessment of the electorate, however. He describes the audience in very active terms, attempting "to tailor the information search to match the realities of media coverage" (p. 72). Unfortunately, there is little evidence of such activity, except that the reading public in Pittsburgh switched to other print sources during the strike. The positive thing we can say about Mondak's sample of voters is that they seem to know where their news is coming from. For example, they more commonly report receiving "a good deal" of television news about the presidential race than they do about the Senate race.

In spite of his results, Mondak is no friend of television. He continually refers to TV news in pejorative terms as an "easy," "passive," "fallback," "free" medium. There is no need, however, to downplay the amount of coverage that the presidential contest receives on network television nor the use to which the electorate puts that information. TV is different from newspapers and in ways that are not all bad. Through television, the audience has a chance to experience something akin to seeing and hearing the candidates and campaign events. Even highbrow news consumers would have difficulty giving up televised coverage of conventions, debates, news conferences, and speeches for a strict diet of print fare. Mondak provides convincing evidence that having "nothing to read" need not mean being uninformed. In fact, the redundancy built into the American news environment makes it possible for people with a variety of tastes and skills to engage in political discourse.

Cityscapes and Capital: The Politics of Urban Development. By Michael A. Pagano and Ann O'M. Bowman. Baltimore, MD: Johns Hopkins University Press, 1995. 188p. $32.50.

Richard E. DeLeon, *San Francisco State University*

Michael Pagano and Ann Bowman have written an informative and provocative book that opens new terrain for students of urban politics and local economic development. Based on a comparative analysis of ten medium-sized U.S. cities and forty development projects, *Cityscapes and Capital* explores the role of image, vision, and leadership in mobilizing public capital to attract private capital to maintain or expand a city's niche within regional, national, and increasingly global urban hierarchies. Although flawed in some ways and limited in scope, this study earns distinction for its attempt to conceptualize the subjective dimension of development politics and to measure the impact of collective hopes and dreams in shaping a city's future.

Drawing upon a wealth of data obtained from site visits, field research, personal interviews, and public documents, Pagano and Bowman argue that "politics matters" in determining how cities adapt to their changing environments. Acknowledging that local governments are constrained by economic circumstances, tax/service ratios, and limited authority, the authors contend that development is not an "autonomic" response to market forces (p. 2). Local officials control many forms of public capital, tangible and intangible, that can be used to induce private capital investment and target the content, location, and beneficiaries of development. Their image of the city and vision for its future place the city into "perceptual orbits" of functional specialization and spatial competition with other cities that market forces alone would not dictate. Further, in some advantaged locations, local officials can choose to spur development where it might not otherwise occur (e.g., Huntsville, Alabama) or thwart development in the face of external pressures to build and expand (e.g., Santa Barbara, California).

Pagano and Bowman devised a useful two-dimensional classification scheme for purposively sampling their ten cities and comparatively analyzing development projects and outcomes in each. Maintenance cities (Boise, Independence, and Santa Barbara) are characterized by low economic distress and low activism in the use of development policy tools. Survivalist cities (Duluth and Evansville) have high distress and high activism. Market cities (Beaumont and Springfield) have high distress and low activism. And expansionist cities (Orlando, Huntsville, and Lowell) have low distress and high activism.

Based on their case studies and probit analyses of project failure or success, the authors draw the discouraging conclusion (p. 104) that officials in economically distressed cities are the ones most likely to fail in development efforts to generate needed revenues. Also discouraging to those who care about local democracy is the finding that low-risk projects with minimal public visibility and controversy have the greatest chance of success (p. 104). Another important finding is that local officials are much more concerned with the tax-and-service impacts and revenue-generating potential of a development project than with its capacity to generate jobs and income for average citizens (p. 3).

Pagano and Bowman's generalizations are limited to medium-sized U.S. cities that are not contiguous with major metropolitan areas. A natural next step would be to replicate the research in medium-sized cities located in major urban centers, particularly with an eye to testing the potency of city self-image, identity, and vision on the development process.

Given the book's theme that politics matters, it is disappointing that so little attention is given to politics in the ten cities studied. As the authors point out, a city's development goals and priorities can shift dramatically with changing political coalitions, election results, and new faces at city hall. Yet the reader will find few descriptions of politics on the input side of the development policymaking process. Nor will the reader find much discussion of the role of business elites or other private sector interest groups in shaping a city's vision and development priorities. Indeed, the authors' emphasis on the public sector and local officialdom seems excessive and begs the question of where images and visions originate if not from the officials themselves.

Too often throughout their book, Pagano and Bowman misleadingly portray the city as a rational unitary actor imbued with a singular vision for the city as a whole. The authors know better, of course, and the criticism here is more one of style than of substance. For example, the authors assert that Santa Barbara has a "penchant for introspection" (p. 41) and that "Huntsville has long felt somewhat estranged from the state" (p. 53). These kinds of linguistic shortcuts are admittedly difficult to resist, but their cumulative effect is to erase conflict and politics from the discussion.

Methodologically, Pagano and Bowman's ten-city comparison fills an important need for studies in the middle range between qualitative and quantitative research. The limitation of this design, however, is that it lacks both the thick history and rich detail of in-depth case studies of single cities and the generalizability of multivariate analyses of large-N sample surveys. Neither fish nor fowl, the study has an inconsistent style and vacillates between spurts of statistical rigor and thin anecdotal vignettes. In addition to their use of conventional tools, the authors might have considered analytical approaches more suited to small-N comparative case studies, such as Charles Ragin's application of Boolean logic in *The Comparative Method: Moving beyond Qualitative and Quantitative Strategies* (1987).

As a final point of criticism, Pagano and Bowman's central concept of "perceptual orbits" is confusing and hard to pin down, especially when it is interlaced with other, more static visual imagery such as hierarchies and niches. Often it is not clear what is orbiting what in the authors' analysis, and the effect on the reader is sometimes unsettling. Although flawed by its overemphasis on the public sector and its scant attention to the political genealogy of images and visions, Pagano and Bowman's study provides a useful conceptual framework and vocabulary to guide future research in this area.

The Impact of Judicial-Selection Method on State-Supreme-Court Policy: Innovation, Reaction, and Atrophy. By Daniel R. Pinello. Westport, CT: Greenwood, 1995. 226p. $59.95.

Henry R. Glick, *Florida State University*

Whether the method of selecting judges has an effect on judges' preference and court decisions has been a controversial issue in the politics of court reform and in political science for decades. Many campaigns have been waged in the states in the hope of removing judges from partisan politics and objectifying their decisions by changing from election to merit selection in which the organized bar and sitting judges have a greater opportunity to impose their values and criteria on choosing judges.

As Daniel R. Pinello points out, scholars of judicial behavior generally have concluded that the method of selection makes little or no difference on who the judges are or the kinds of decisions they make. But he relies on political theory supporting the differential effect of the method of selection on judicial behavior and he believes the relationship deserves another, closer look. He hypothesizes that elected state supreme court judges are more likely than others to be influenced by public opinion and, in contemporary politics, to favor the state over individuals and business; but when the state is not involved, judges favor the individual over business. Gubernatorially appointed judges are less constrained and prefer business over the individual and the state, or individuals over the state. Legislatively selected judges, who usually are former legislators, have no constituency and suppress their preferences in favor of the policies of other branches of government. Pinello tests relevant sub-hypotheses by examining decisions in business law, criminal procedure, and family law. Overall, he finds relationships that confirm most of his hypotheses and support the claim that how judges are chosen makes an important difference in what they do on the bench. His discussion regarding legislatively selected judges as passive policymaking is especially interesting and suggestive since there are only three states that use this method of selection and there has been very little previous research on these courts.

Pinello generally is clear in explaining how he has conducted this research and he explores (but rejects) alternative explanations for the findings. However, the methodology and its execution kindle reservations about the conclusions. Rather than examine a large sample or a universe of cases in each of these fields of policy in a number of states, Pinello uses a matching strategy to compare the decisions of three pairs of state supreme courts whose judges are chosen by different methods but purportedly are similar in other ways, thereby "holding constant" other forces that could influence the content of decisions. This is an innovation in judicial research, but Pinello matches the states according to political party structure and geographic proximity, not legal culture, jurisdiction, judicial innovation, or reputational scores, which are structural and behavioral variables that plausibly also could influence the content of court decisions. This particular matching strategy seems to overlook important forces uncovered in previous theory-driven empirical research on state courts.

Pinello adopts a very stringent method for selecting cases for analysis; perhaps it is too stringent, for it eliminates significant amounts of data. He examines six cases per state court in each area of law, gathered over a span of approximately 30 years. Among the many criteria, the cases had to involve noteworthy policies (i.e., were discussed in legal periodicals), present clear dichotomous options, involve judicial innovation, and produce opposite decisions within pairs of courts.

He provides another list of more than five dozen cases that were considered but did not fit the criteria. However one-third of these were rejected because all of the six courts made *similar* decisions in the three areas of law, and several others were deemed not factually comparable, were too complex, or did not present clear support for one or the other litigants. Although consistent with the methodology, eliminating these additional cases forces Pinello to rely on a very small number of decisions to support his unconventional conclusions. If a larger number of cases were included, courts would appear to have made many similar decisions in these policy fields regardless of method of selection. In addition, several of the paired cases he included in the data set were decided nearly a quarter of a century apart or contain factual differences that may be relevant to the courts' decisions, thereby suggesting additional variation over time and within the paired states that possibly could explain the decisions.

Pinello claims that it is questionable to reduce judicial opinions "to meaningful and accurate quanta of usefully manipulable data" (p. 11). But this statement is perplexing because he codes court opinions according to the issues and decisions favorable to the litigants and an appendix includes a series of two x two contingency tables with correlations and tests of significance used to support the hypotheses. Despite this quantitative analysis, Pinello states that these few cases should be considered *examples* of judicial action but that including a larger number of cases would not reveal more information. Moreover, he argues it is impossible to select a random sample of court cases for analysis, but the reasons for this uncommon conclusion are not made entirely clear.

Daniel Pinello has made a contribution by employing a matched pairs methodology for studying state courts, and the potential of this approach for state politics research needs to be explored more fully. This general approach could supplement the results produced by larger aggregate studies. Certain of Pinello's arguments are suggestive and require more research, but his particular research strategy, particularly his decision to scrupulously limit the number of cases for analysis, restricts the challenge he can make to previous work on this controversial subject.

Community, Culture and Economic Development: The Social Roots of Local Action. By Meredith Ramsay. Albany: State University of New York Press, 1996. 163p. $16.95.

Gerry Stoker, *University of Strathclyde at Glasgow*

This book presents an account of rural politics in two small towns in Somerset County, Maryland's poorest county. It argues that there are considerable economic and political interests that favor the status quo in these two towns, contrary to the conventional wisdom that such communities will invariably pursue outside investment and growth. To explain the resistance to modernization and change, Meredith Ramsay draws on insights from cultural theory and the urban-based regime and growth coalition theories. She ultimately offers an explanation that combines an understanding of context with an analysis of the dynamics of power within such communities and the strength of core community values. Along the way she provides a fascinating and highly readable account of small-town politics in the United States. What is perhaps not so clear is whether, as she claims, her book has a wider relevance beyond the immediate objects of study.

The book opens with a preface that provides a refreshingly honest account of the trials and tribulations of conducting research. Community-based research is difficult enough, but issues about being a white woman researcher in an insular society structured by race and gender added to the complexity of task. Dr. Ramsay started out in Princess Anne using traditional methods to collect documentary evidence and sought to supplement this with interviews. Unfortunately, many of those she interviewed were patronizing, others failed to keep appointments, and still others thought she was a federal agent and kept their distance! Still, she persevered and changed tack by showing up at public events and striking up conversations. Access was also sought in her second case study of Crisfield. Slowly but surely the dynamic of communities began to reveal themselves. She gained the trust of key individuals and in her own estimation "hit pay dirt" (p. xv). The veracity of this claim is amply supported in the two case study chapters that form the heart of the book.

Chapters 1 and 2 provide the theoretical background, chapter 3 looks at the historical context, and chapter 6 provides the conclusions. Chapter 4 provides an account of the politics of Princess Anne during 1986–91, while chapter 5 examines the experience of Crisfield over the same time period. What do these two case studies tell us?

For some time the decline of agriculture and wider processes of economic decay had left Princess Anne facing a steady exodus of businesses and rising unemployment. The financial pressures of the town encouraged an interest among elites in economic growth, but this impetus was held in check by a concern that new businesses would undermine the low-wage, labor-intensive nature of existing industries. The long-standing elected leaders, along with some incoming private business, finally took the plunge and went for growth in 1986. Unfortunately for them, these growth policies proved unpopular. A combination of an empowered electorate and middle-class white voters who had "retired" to Princess Anne for a quiet life managed to oust the established leaders in 1990. However, the newly elected leaders were subjected to considerable harassment by forces associated with the old regime. Neither the challengers nor the established groups were able to exercise dominance. The result was a rather unpleasant stand-off between new democratic forces and a white male elite prepared to threaten extralegal violence in order to ensure that its position was not undermined.

In Crisfield the social and political story took a rather different path. It was a community founded by dissidents and marginal persons, in part in defiance of the plantation hierarchy that dominated elsewhere in Somerset County. Its people were proudly independent and have remained so. Their community was poor, but there was a fundamental commitment to the lifestyle that had developed. Some business interests tried to stimulate growth, but the town and its political leadership remained wedded to a subsistence economy that delivered for them a supportive and tolerant community in which they felt at home.

So far then the book looks set to provide the basis for a good movie, but does it contribute anything to political science? On the positive side it provides a rich account of rural town politics and rightly chastises those who neglect such issues. Anyone interested in the practice of contemporary American democracy cannot ignore the evidence produced by Dr. Ramsay. The book also nicely brings into focus the moral dimensions to the social and political choices made by individuals. It supports those in political science who argue for the need to look beyond self-interest in explanation. Finally, it challenges the naïve view that economic development is universally a good thing and explains clearly and rationally why some communities might resist it.

Less convincing, however, is the attempt to use regime theory as a framework for the study. A regime is not just any coalition; it is a particular form of coalition based on a merging of public and private interests to establish a long-term dominance over the policy agenda of a community. A regime operates outside the ambit of normal representative politics and relies on the strategic and deliberate interventions of a mixed group of public and private actors, each with their own institutional base to achieve its purposes. Dr. Ramsay claims that a growth machine was constructed in Princess Anne and a subsistence regime was maintained in Crisfield. According to my understanding of regime theory and the evidence presented by Dr. Ramsay, I would disagree with that judgment. I suspect that in her two areas there were no effective regimes and rather one shaky (Princess Anne) and one firmly based (Crisfield) governing coalition of elected leaders. What is disappointing about Dr. Ramsay's study is that having raised the issue of whether urban regime theory can be transplanted to a rural setting she fails to get to grips with the theoretical work that is required to tackle that issue. Her work is insightful but not sufficiently theoretically grounded in this respect.

The Jurisprudential Vision of Justice Antonin Scalia. By David A. Schultz and Christopher E. Smith. Lanham, MD: Rowan and Littlefield, 1996. 245p. $62.50 cloth, $23.95 paper.

Richard A. Brisbin, Jr., *West Virginia University*

Four decades ago the Supreme Court's justices were strong personalities with distinctive visions about the future of the American regime. Justices Hugo Black, William Brennan, William Douglas, Felix Frankfurter, John Harlan, and Earl Warren held a great seminar on the rights and powers of government. Since the retirement of Brennan and Thurgood Marshall, the current Rehnquist Court has become populated by bland personalities with no clear political vision and an eagerness to parse statutory language. The exception is Antonin Scalia. Like justices of four decades ago, Scalia has adopted a combative style to put his personal impress on American law and politics. What other Rehnquist Court justice would allege that his colleagues want to be a "junior varsity Congress"? What other justice would complain in an abortion case about a "Nietzschean vision of us unelected, life-tenured judges—leading a Volk"? What other justice would assert in a gay rights case that "The Court has mistaken a *Kulturkampf* for a fit of spite"?

David Schultz and Christopher Smith have drawn together their previous articles about Justice Scalia to sort out the content of his political message and his vision for the future of the nation. The strength of their study is its recognition that Scalia has a unique approach to constitutional issues that they identify in their first chapter as a "post-*Carolene Products* jurisprudence." Using Scalia's Takings Clause opinions, they emphasize how the justice has developed a distinctive set of standards that modify or abandon the tiers of scrutiny formula developed by the Court in equal protection, First Amendment, and economic cases during the post–New Deal era. Although more could be said about Scalia's abandonment of higher tier scrutiny doctrine in racial discrimination cases (*City of Richmond v. J.A. Croson Co.; Adarand Constructors v. Pena*), the authors have identified a core principle that influences Scalia's interpretative process. For example, a chapter on religious liberty presents a fair if somewhat apolitical interpretation of the application of scrutiny doctrine in his opinions. In their discussion of free speech the authors appropriately note how no single scrutiny formula or other consistent doctrinal standard has marked the justice's opinions. Their chapter on criminal justice clearly points out the dedicated majoritarianism of Scalia's procedural due process jurisprudence, an indirect reference to his refusal to give any variety of heightened protection to procedural rights.

This book, however, has problems in its theoretical focus, organization, and coverage. The authors draw a distinction between a political analysis and a jurisprudential analysis of Scalia in their opening chapter that parallels the distinction between the attitudinal and legal explanations of judicial choice. They propose an eclectic mix of these explanations in their study, but actually they provide only limited analysis of the dimensions of Scalia's conservative ideology. The inattention to Scalia's ideological conservatism means that, unlike Smith's (1993) *Justice Antonin Scalia and the Supreme Court's Conservative Moment*, there is marginal attention paid to the ideological direction of Scalia's votes, save on freedom of speech and criminal justice, and to the political assumptions and objectives embedded in his "post-*Carolene Products* jurisprudence." When examining Scalia's "legitimate jurisprudential reasons for his claims" (p. xxiii) the authors do not fully consider whether a constitutive political ideology undergirds his arguments. Moreover, when they discuss Scalia's ideology, as they do in a section on administrative law, the content of his ideology is not precisely defined.

The authors' interpretation of themes in Scalia's jurisprudence other than post-*Carolene Products* rights doctrines also need more careful consideration. One theme deserving closer analysis is the justice's prescription for the distribution of governmental power. Contrary to the views of these authors, Scalia is not concerned with whether Congress should be a "weak" or a "strong" institution (p. 33) but with whether it ought to be compelled to draft statutes with more precise language, and especially with the powers delegated to bureaus. Closer study of Scalia's pre-Court articles might have led to a better understanding of this dimension of his political message.

The organization of the book is scattershot. Other than the relatively brief introduction and conclusion, the book is an edited compilation of previously published articles. The result is some overlap: for example, the discussion of separated powers is partially duplicated in three separate chapters on constitutional interpretation, statutory interpretation, and institutions. Curiously, First Amendment cases related to partisan activity are in a chapter largely devoted to separated powers issues. There is a clear disparity in the style of presentation among the chapters, and there are differences of emphasis on the post-*Carolene Products* jurisprudence. For example, the centrality of the post-*Carolene* argument in the early property rights chapter disappears in the later free speech and criminal justice chapters. The book's coverage of Scala's activity also has significant gaps. The justice's critical abortion opinions and his position on substantive due process arguments in punitive damages opinions receive barely a nod. Racial and related class-based discrimination cases are intermingled with discussions of other topics and do not receive concentrated attention. Federalism issues, including Scalia's unique views on the dormant commerce clause, also get short shrift. Except for discussion of religious liberty cases and a brief survey of his "originalist" arguments, tradition, a favorite term in Scalia's opinions, passes without close analysis. Finally, the authors fail to compare the justice's jurisprudence with contemporary jurisprudential argument, conservative political thought, and the arguments of his colleagues in any special section or at any length. Such comparisons would establish a stronger basis for their conclusion, very briefly argued, that Scalia is a "different kind of conservative."

As a first comprehensive effort to explain Scalia, clearly this book is the place to begin a study of the justice. It is especially useful as a guide to the central themes of Scalia's opinions. However, because of the disjointedness of the authors' theoretical focus and presentation and their failure to explore the fit of Scalia with contemporary conservatism, persons interested in the justice also need to read materials listed in their excellent bibliography to get a more compete sense of the political vision of this forthright jurist.

Managing Legal Uncertainty: Elite Lawyers in the New Deal. By Ronen Shamir. Durham, NC, and London: Duke University Press, 1995. 304p. $45.00 cloth, $18.95 paper.

Mark Kessler, *Bates College*

In this richly detailed sociohistorical study, Ronen Shamir (sociology, Tel Aviv University) examines conflicting political reactions of elite corporate lawyers, solo practitioners, and academic legal realists to the New Deal's legislative program. Challenging the common view of lawyers as clients' "hired guns," the study situates legal advocates in a diverse and competitive professional environment that shapes their practices. The theoretical framework draws on Weber and Bourdieu's sociology of law, using Bourdieu's notion of the "legal field" to emphasize the differential positions, tasks, and interests of various segments of the legal profession and Weber's concept of "social closure" to suggest that different segments of the bar use law to claim a specialized knowledge and expertise in order to secure their own market advantages. Throughout the book, Shamir argues that lawyers are motivated first and foremost by self-interested concerns, such as maximizing their own legal market positions, political power, and personal prestige.

Several intensive case studies are presented to illustrate this major theme. For example, Shamir shows how corporate lawyers took positions different from their corporate clients, opposing the National Industrial Recovery act (NIRA) and supporting its invalidation by the United States Supreme Court in *Schecter Poultry Corporation v. United States*. Large corporations supported provisions in the NIRA allowing trade and industrial associations to create "codes of fair competition" because they believed that they would use this process to secure advantages for themselves. Corporate lawyers, however, opposed administrative features of this legislation because the expansive discretionary powers conferred on administrators threatened their own professional authority. Furthermore, Shamir shows how solo practitioners, unlike large firm corporate lawyers, welcomed the growth of administrative agencies during the New Deal as a possible source of new professional opportunities. The practical political significance of one's location in the legal field is shown by the dramatic contrast between solo lawyers' vigorous support for legislation restricting administrative law practice to lawyers and corporate lawyers' indifference and general noninvolvement in this issue.

In a chapter on the legal academy, Shamir portrays academic realists as engaged in a struggle with elite corporate lawyers over professional status and influence within and outside the legal field. According to Shamir, legal scholars developed realism's critique of law's certainty to overcome the isolation and powerlessness of the legal academy in society. By providing legal analyses supporting administration policies and helping to formulate some New Deal political reforms, academic realists enhanced their general status and influence. And by incorporating social scientific theories and methodologies, the realists bolstered the relatively feeble position of legal scholarship within the academy.

The analysis is at its best when depicting the complex environment in which elite lawyers operated and the complex responses that resulted. For example, Shamir shows how corporate lawyers sought to criticize the NIRA without offending their clients; how they opposed it without directly entering partisan politics, a move that would have damaged their image as legal authorities; and how they were careful to express reservations about administrative elements of the legislation while simultaneously supporting the use of heroic measures in an emergency atmosphere. Rather than directly positioning themselves in opposition to New Deal programs that the public and their clients may have supported, corporate lawyers expressed their concerns through the American Bar Association on the grounds of seeking to protect the interests of the law and legal system. They consciously avoided any arguments offering positions on more clearly identifiable, substantive distributive issues.

The analysis is least satisfying when the complexity of lawyers' practices is collapsed into one major motivation for action—lawyers' self-interest. After presenting a richly textured and nuanced description of the NIRA case, for example, Shamir draws the following conclusion: "It is impossible to reduce lawyers' ideas and practices merely to the expectations of clients or to an extralegal commitment to laissez-faire ideology. Rather, the case of the NIRA allows us to identify the particular worldviews and distinct concerns that are bred by the structure of the legal field and the particular position of corporate lawyers within it" (p. 34). Ultimately, Shamir views all arguments and policy positions as entirely strategic, shaped exclusively by one's structural location in the field. For example, lawyers' efforts to distinguish between law and politics—criticizing the administrative process created in legislation, rather that the legislation's substantive political and economic impact—is viewed as "not based on a naive formalist vision of law as a separate universe of discourse" (p. 80). Instead, the distinction serves the strategic self-interest of lawyers by creating a symbolic separation between law and politics necessary to maintain an aura of professionalism that perpetuates the profession's social and economic privileges. While this interpretation may at least partially explain positions taken by some lawyers at this time, the analysis largely ignores potentially important sites outside the particular structural position of a given bar segment where ideologies are produced, shaped, and reinforced. Studies of legal training in law schools, for example, suggest that lawyers of all kinds practicing in every segment of the profession are trained to make the distinction between law and politics. Indeed, this very distinction is a crucial element in what Granfield (1992), in *Making Elite Lawyers*, calls the "ideology of professionalism" (p. 74). Thus, even before occupying a specific structural location in the legal field, contemporary law students learn an ideology that may promote the self-interest of only some future practitioners.

Like most explanations relying on individual or group self-interest, this analysis is open to the criticism that some deep, hidden selfish motivation can always be asserted to explain what appears to be the most public-regarding action. Shamir's interpretation of academic realists, for example, is unlikely to persuade those who view them as important reformers interested in promoting the public good. And questions may be raised regarding whether the study's theoretical framework applies to other segments of the legal profession at other historical moments. How does Shamir's use of the legal field concept help us understand the practices of lawyers in the public sector? What position do prosecutors, legal aid lawyers, and attorneys for interest groups occupy in the legal field and what specific interests do they promote? Does the legal field help us understand the practices of lawyers for the NAACP and their opposition to segregation? Such questions regarding the framework's broader applicability are not addressed in this volume.

Despite these reservations, Shamir provides a provocative and, at times, compelling explanation of legal practices during the New Deal. The study demonstrates the potential significance of lawyers' structural location in a highly competitive environment for understanding the roles that different types of lawyers play in national politics. In its treatment of lawyers during the New Deal, it adds importantly to our knowledge of the legal profession's development and evolution. Theoretically, Shamir's analysis leaves us with the prospect of developing Bourdieu's metaphor of the legal field to incorporate more complex understandings of the political roles and ideological practices of a wide variety of lawyers. Legal scholars would be well advised to read this study carefully before embarking on this important journey.

Madison on the "General Welfare" of America: His Consistent Constitutional Vision. By Leonard R. Sorenson. Lanham, MD: Rowman and Littlefield, 1995. 178p. $58.50 cloth, $22.95 paper.

George W. Carey, *Georgetown University*

In recent decades some scholars have challenged the widely accepted position that Madison's views on federalism, particularly with respect to the constitutional distribution of powers between the states and the national government, were far from consistent over the course of his political career. It is widely believed, for example, that Madison broke with Hamilton and abandoned the cause of a strong central government shortly after operations began under the new Constitution. Moreover, conventional wisdom holds that Madison, having made this switch, did not consistently hold to his position that strictly limited the powers of the national government.

Sorenson is among those who believe that Madison was consistent throughout his political career under the Constitution, and a goodly portion of this work is devoted to arguing this point. Sorenson's announced mission, however, and one which occupies his attention primarily in the opening chapters, is to refute William Crosskey's charge that Madison is guilty of a deception of the first order. Briefly put, Crosskey's thesis is that the Framers intended to construct a consolidated or unitary system. In his view, then, the "general welfare" clause is a substantive power that gives the Congress virtually unlimited legislative authority. As Crosskey would have it, Madison understood as much but sought to deceive the people about the meaning and purpose of the clause. The most notable deception, according to Crosskey, occurs in *Federalist* 41 where he claims that Madison interprets the clause as "neither an independent substantive grant of indefinite general power to Congress nor a statement of ends" (p. xii). Using Crosskey's contentions as the primary foil, Sorenson shows in some detail how Madison's understanding of the general welfare clause has been distorted. Above all, however, he wants to demonstrate "that Madison held throughout his career the position put forth in *Federalist* 41, namely, that the primary purpose of the enumeration is to give a more precise "definition" of the ends, or "objects," entailed with the "common defense and general welfare" clause; that while the clause does involve ends and "does authorize particular powers other than those enumerated," it is not an "open-ended" clause nor does it grant "plenary" powers to Congress (p. xiv).

Contrary to Crosskey's interpretation, Sorenson understandably insists throughout that Madison did not hold that the general welfare clause should be defined exclusively in terms of the following enumerated powers, which, in effect, would render it superfluous. He also remarks that the clause "does not convey to the Congress a general, independent, substantive grant of undefined, indefinite, power for indefinite ends," that is, a grant of power that would render the subsequent enumeration superfluous. Instead, as Sorenson would have it, Madison's position assumed a middle ground, namely, "any new particular power derived from it [the general welfare clause] must be done indirectly through examination of the enumeration, which also defines more precisely the ends of the enumerated powers" (p. 103). Or, put otherwise, "Madison held one consistent position: that the Constitution is limited to a limited number of limited ends: that the enumeration more precisely defines those and that the general clause vests particular powers beyond the enumeration for those ends, which can only be determined indirectly through an examination of the enumerated powers themselves in relation to those ends" (p. 104). Yet, Sorenson is niggardly in providing concrete examples of powers, though confined, that Madison derived from the general welfare clause. Nevertheless, one instance—Madison's claim that "the common defense and general welfare embraces or entails the 'purpose' of the 'encouragement of domestic manufacturers' " (p. 91)—does raise serious questions that Sorenson does not explore about how to apply Madison's middle ground approach in a principled manner.

Sorenson is quite correct in noting that his inquiry involves basic principles that determine the character of our constitutional regime. While his defense of Madison is both interesting and provocative, students today may well decide that his epilogue, which lucidly examines the steps by which the Supreme Court has in effect come to embrace the Crosskey position regarding the meaning of the general welfare clause, is the most fascinating and informative part of the book. "The Court," he writes by way of conclusion, "has practically confirmed that the Constitution establishes, for the American people, a virtually unlimited government: a government constitutionally authorized to promote, with the consent of the American people, any end it desires and to provide for that end with practically unlimited power" (p. 168). Sorenson has no difficulty in setting forth a "hypothetical Madisonian assessment of these post-Madisonian developments": "The Court's construction of the general welfare clause constitutes an unconstitutional and potentially revolutionary construction of the Constitution, a construction that reaches to and transforms the very nature of our Constitution" (pp. 168–9).

Yet might not Madison—as he did in 1817 on the question of the constitutionality of the national bank—fall back to the position that "in certain circumstances the Constitution itself authorizes that it be expounded by means of 'precedents' " (p. 60)? After all, the New Deal Court, in rendering an expansive construction, was in a sense only ratifying what the political branches had affirmed, and over a sixty-year period subsequent presidents and Congresses of both parties have reaffirmed this interpretation. Moreover, during this period it has been accepted alike by the people and leading constitutional law scholars. In other words, using the tests of established precedent set forth by Madison to uphold the constitutionality of the national bank, personal views concerning original intent would have to be set aside as wrong in light of such lasting and widespread consensus on a contrary position.

Sorenson may well be correct in maintaining that Madison would look with alarm at modern developments. Yet the fundamental questions raised, but not explored, by his analysis revolve around what kinds of issues ought to be settled by the precedent tests and which by reference to original intention. When, for example, ought precedent to trump original intent or vice versa? The value of his work resides as much in pointing up these perplexing difficulties as it does in trying to render Madison consistent.

Popular Government and the Supreme Court: Securing the Public Good and Private Rights. By Lane V. Sunderland. Lawrence: University Press of Kansas, 1996. 361p. $40.00.

Gary Jeffrey Jacobsohn, *Williams College*

One of the frustrations of writing about the world's oldest written constitution is that there are certain subjects about which it is very difficult to say anything that has not been said before. Contemporary commentators on the role of the Supreme Court in a constitutional democracy can perhaps take perverse comfort in the fact that it is not just intellect that distinguishes their efforts from those of a Joseph Story, but rather the unfair advantage that this worthy predecessor enjoyed in writing about such a relatively new document. In the case of Lane V. Sutherland and his book on the Supreme Court and popular government, the knowledgeable reader should not expect to encounter a novel or especially original treatment of this much considered topic. But given the importance of the issues at stake in these considerations, thoughtful revistings of well-trod scholarly terrain may still produce ample intellectual reward.

Sunderland believes that the Supreme Court's role in American politics should conform to the republican principles underlying the constitutional system. These principles, he maintains, require an acknowledgement of the supremacy of the legislative power, which is to say that judicial authority ought to be much more carefully circumscribed than it has been in recent times. "People want, and are entitled, to control their politics through representation. This is the essence of republicanism and the fundamental principle of the United States Constitution" (p. 84). Those who see the Court as the principal guardian of individual rights (including liberals like Ronald Dworkin and Laurence Tribe, as well as such conservatives as Robert Bork and Richard Epstein) fail to appreciate the wisdom of *The*

Federalist (and the Lockean natural rights philosophy upon which it rests) that "a dependence on the people [is] the primary safeguard of private rights." Rightly understood, majority rule and minority rights are not antagonistic ideas; within the framework of republican principles they are, Sunderland argues, perfectly compatible.

In the first part of his book Sunderland examines with considerable sophistication the theoretical foundations of American constitutionalism. The teachings of Hobbes, Locke, Montesquieu, *The Federalist,* and the Declaration of Independence are skillfully analyzed to reveal an essential core of republican philosophy that can illuminate contemporary debates over the exercise of judicial power. Again, there is very little new here, but the author is to be commended for rendering one of the clearest explications in the scholarly literature of these critical sources as they apply to the role of the courts. He is particularly effective in establishing the doctrine of natural rights as the essence of republican government, demonstrating through this exercise that popularly based law need not be associated only with the positive law of the state. In so doing, he forces one to take seriously the proposition that it is in "the structure of government and a written Constitution, rather than enlightened statecraft [including judges as peculiar guardians of an ever-expanding catalog of individual rights]," that one finds the primary security for the natural rights outlined in the Declaration of Independence (p. 33).

The second part of the book consists of a series of critiques of leading figures in contemporary constitutional jurisprudence, with each instance becoming an occasion for Sunderland to reiterate his thesis. While often quite insightful, the cumulative effect of this effort (which better editing could have minimized) is to wear down the reader with repetition. But what becomes clearer as one reads on is that in his devotion to republican ideals, Sunderland displays a commitment to jurisprudential purity that arouses the same sort of skepticism that Madison felt toward the "theoretic politicians" of his day. One can be broadly sympathetic with the author's account and yet question the reasonableness of the absolute priority assigned to legislative supremacy.

Sunderland argues, for example, that "the American political order is so fundamentally democratic that, in the long run, popular will prevails. Any theory of the judicial power that does not recognize this as fundamentally important misunderstands the appropriate role for the Court" (p. 134). To demonstrate the inappropriateness of entrusting to the undemocratic Court the enforcement of principles of natural right, he cites the horrendous decisions in *Dred Scott v. Sandford, Plessy v. Ferguson,* and *Buck v. Bell,* citations that prove the Court's incapacity to perform as a "prophet of moral growth" (p. 157). But in fact these cases do not advance the claim being made, that the only defensible way of respecting the natural rights commitments of the founders is to defer to the republican will as expressed through the legislature. *Buck,* for example, is such an egregious case because that is precisely what Justice Holmes did, refusing to find in the state's enforced sterilization policy a violation of fundamental liberty.

It is true, as Sunderland notes, that Alexander Hamilton found nothing in the Constitution that "*directly* empowers the national courts to construe the laws according to the spirit of the Constitution." But he does not note that previously Hamilton had averred that "all lawyers agree that the *spirit* and *reason* of a law, is one of the principal rules of interpretation." As to the abhorrent abuse of power revealed in the State of Virginia's sterilization policy, would Hamilton's republican commitments have prevented him from supporting a judicial application of unenumerated natural rights to nullify this legislative monstrosity? It was, after all, Hamilton who wrote: "If the constitution were even silent on particular points, those who are intrusted with its power would be bound in exercising their discretion to consult and pursue its spirit, and to conform to the dictates of reason and equity."

Sunderland appropriately criticizes Robert Bork for his slighting of natural rights theory, insisting correctly that the "framers were not legal positivists" (p. 198). While agreeing with most of Bork's conclusions on constitutional issues, he nevertheless accuses Bork of "shar[ing] in the mainstream's rejection of reliance on the strictly republican character of the Constitution and that mainstream's adoption of judicial supremacy" (p. 200). In the final analysis, however, Sunderland's own adoption of a rigidly procedural model of natural rights jurisprudence, in which the dictates of republicanism are understood to mandate all judicial avoidance of unenumerated rights, leads ineluctably to an effectively positivistic solution, one with which Bork's more straightforwardly positivistic jurisprudential approach would have little to quibble.

Remaking Congress: Change and Stability in the 1990s. Edited by James A. Thurber and Roger H. Davidson. Washington: Congressional Quarterly, 1995. 217p. $38.95 cloth, $24.95 paper.

Paul J. Quirk, *University of Illinois at Urbana-Champaign*

The U.S. Congress in recent years has refused to sit still long enough to let political scientists paint an accurate portrait. Over the past two decades Congress has been embroiled in several periods of reform, actual or attempted. The seemingly stable Congress of the post-Cannon era was followed in rapid succession by a reform Congress in the mid-1970s, a postreform Congress in the 1980s, a new round of institutional turmoil and reform ferment in the early 1990s, and major changes resulting from the Republican takeover in the 1994 midterm election. Complicating analysis, the reforms and proposals have sprung from contradictory impulses, even in the same period, and the Senate and House have differed considerably in their responses. This well-conceived and exceptionally useful collection of essays helps sort out the politics of institutional change in Congress, with emphasis on recent decades and especially on the two most recent Congresses.

Through a combination of fortunate timing and conscientious effort, the collection manages to be current despite the rapid developments in its subject. The authors were assembled largely to reflect on the reform efforts of the 103rd Congress (1993–94) at the Conference on Congressional Change at American University in October 1994—the month before the midterm election gave congressional Republicans majority status and the opportunity to define and partially implement anew reform agenda. To rescue the project from immediate obsolescence, most of the essays were revised or expanded months later to take into account the dramatic institutional developments early in the succeeding 104th Congress. The book provides thorough and roughly comparable treatment of the 103rd and 104th Congresses. Barring further upheaval in the next Congress, it will have a long shelf life.

After an introduction by co-editors James Thurber and Roger Davidson, the collection includes a chapter on the history of institutional change in Congress (by Leroy Rieselbach); analyses of efforts to reform the committee system

(Davidson), floor procedures (Sarah Binder and Steven Smith), leadership roles (Barbara Sinclair), the budget process (Thurber), campaign finance (Candice Nelson), and lobbying (Ronald Shaiko); and accounts of the Joint Committee on the Organization of Congress (C. Lawrence Evans and Walter Oleszek), public opinion toward Congress (Norman Ornstein and Amy Schenkenberg), and the Renewing Congress Project of the Brookings Institution and the American Enterprise Institute (Thomas Mann)—along with a concluding essay (Davidson). All of the essays are more than adequate, most are strong, and some are distinguished.

The book is not primarily concerned with addressing academic debates about legislative organization. There are few or no references to such matters as structure-induced equilibrium, legislative delegation, distributive versus informational perspectives, or partisan versus individualist models. Although two fine essays (Binder and Smith's and Sinclair's) present very interesting theoretical analyses, most of the chapters are appropriately concerned with developing cogent, theoretically eclectic accounts of the events. Some chapters also make near-term projections or draw practical implications for future reformers.

Undergraduate readers will find the book hard going. Although the essays are well written and edited, the issues of congressional reform are unavoidably technical: whether the House minority can make a motion to recommit with instructions, whether signatures on discharge petitions will be published, whether debate will be permitted on the motion to proceed, and so on. And all that is without mentioning the budget process! The volume will be most useful for scholars, graduate students, and the Washington community.

The view of reform politics that emerges from these essays is complex. Demands for change reflect, among other forces, popular distrust of Congress and politicians, the desire of the members to make Congress an effective and reasonably efficient decision-making body, their equally important desire for participation, efforts by each party to facilitate achievement of its respective policy goals, concern for improving congressional performance on politically tough issues like deficit reduction, and even some concern (at least for public consumption) about neutral notions of fair procedure. This melange of forces can produce seemingly incoherent results: Congress opens up committee processes (through publication of committee votes) and at the same time seeks to avoid accountability (through procedural constraints on budgeting). The most important changes actually accomplished were a moderate consolidation of the committee system and a considerable centralization of agenda-setting in the House—almost entirely under Republican sponsorship in the 104th Congress.

Political scientists can applaud the role that academic students of Congress have played in the reform process, the subject of Mann's essay. Through the Renewing Congress Project, Mann, Ornstein, and others had a semi-formal role of organizing research, analyzing issues, providing advice, and even helping to negotiate recommendations. In doing so, they strove to resist the punitive and populistic notions of reform that prevailed in public discourse. They pressed Congress to protect its diminishing capability for deliberation and to struggle against excessive, not insufficient, response to mass opinion.

One cannot, however, be pleased with the book's implications for the future of Congress. "Congress bashing," as Ornstein argues, is unlikely to abate significantly. If anything, public cynicism about Congress, government, and politicians may grow deeper. The media and opinion environment makes principled arguments for limiting openness or public participation politically untenable, the influence of demagoguery is likely to grow, and the intensification of partisan conflict shoves neutral concerns about institutional capabilities into the background. Each chamber is likely to preserve or exaggerate its respective form of partisan abuse.

Advocating unpopular, seemingly idiosyncratic positions on institutional issues, without much success, may be something political scientists will have to get used to.

Counties in Court: Jail Overcrowding and Court-Ordered Reform. By Wayne N. Welsh. Philadelphia: Temple University Press, 1995. 265p. $49.95 cloth, $22.95 paper.

William A. Taggart, *New Mexico State University*

During the last quarter-century, most state governments and countless local governments have been on the losing side of lawsuits brought by individuals challenging the conditions of publicly operated prisons, jails, and other confinement facilities. Government officials have either been required by court order or consent decree to embark on a course of institutional reform design to ensure the provisions of correctional services meeting some judicially prescribed minimum standards of incarceration. Typically, these orders or decrees are comprehensive in coverage, detailed in application and intrusive by nature, addressing a broad range of policy and administrative issues across numerous areas historically reserved to the authority of the legislative and executive branches. Despite substantial interest from scholars toiling in a number of academic disciplines, our knowledge concerning the causes and consequences of court reform is limited in two significant ways. First, there is a dearth of investigations dealing with jail reform, as the bulk of attention has focused on state-managed prisons. Second, except for case studies lacking in generalizability, much of the existing research has been narrowly construed, examining particular aspects of reform instead of seeking to develop an overall picture of institutional reform as an ongoing, dynamic process reflecting a confluence of actors, events, and forces.

Counties in Court by Wayne Welsh represents a carefully crafted attempt to address both of these glaring deficiencies in the literature. Based on his dissertation research completed at the University of California, Irvine, and a number of previously published articles, Welsh concentrates his attention on county-run jails, employing an open-systems approach that views court reform as an interconnected, sequential process spanning five stages or phases. Each stage encompasses a combination of internal and external inputs, actors, constraints, and outputs that reflect a complex web of causal structures underlying the reform process. Each stage suggests a number of important hypotheses that Welsh is able to examine using cross-sectional and longitudinal data derived from multiple sources, with the preponderance of evidence pertaining to counties in the state of California between 1975 and 1989. During this time frame, 35 out of 58 counties in California were under court order, providing an excellent opportunity to engage in comparative analysis within and across groups of counties. To supplement the empirical information, Welsh also reports on three counties he examined as case studies.

Welsh's book is organized to offer no surprises, though it is somewhat disappointing in two minor respects in the beginning. The first chapter presents a brief overview of jail (as opposed to prison) litigation, introduces the model used to guide the subsequent analysis, and discusses the study's research design. This introductory chapter is rather short, especially in terms of theoretical development and coverage of methodology. In the latter case, the treatment is very terse and the reader is referred to an appendix for additional information, some of which is crucial. It was therefore necessary on a few occasions to flip between chapter 1 and the appendix as findings were presented in later chapters. In addition, anyone looking for a strong theoretical framework at the beginning of the book will be frustrated. Each stage is described in just a single paragraph. Furthermore, except for a couple of pages dealing with selected approaches to the study of court reform, Welsh does not make a strong case for the approach he has adopted, either by comparing it to the alternatives or by linking it to other policymaking theories.

Chapters 2 through 6 correspond to Welsh's five stages of litigation: trigger, liability, remedy, postdecree, and impact. These chapters begin by highlighting some of the major research questions associated with a given stage in the reform process. Welsh moves rather quickly to a discussion of relevant literature and begins weaving his data around the propositions suggested by this review. Simple tables are used freely in each of these chapters to summarize the strength and direction of the many relationships examined. Chapter 7 provides a concise summary of the major findings by stage and considers some implications related to law and social change.

The richness of Welsh's book is discovered in the county-level data he has compiled and how this information is meticulously triangulated to examine multiple propositions associated with the dynamics of reform. Many of theses issues are considered in the literature but have not been subject to systematic evaluation. Welsh tackles these topics using secondary statistical data, personal interviews, and a content analysis of court documents. His content analysis, which is especially noteworthy offers a chronology of legal events on several dimensions for all 35 counties under court order. As part of his analysis of the liability stage, for instance, he is able to document important differences in specific violations alleged by plaintiffs across cases, the legal bases of their independent claims, and the types of defendants named in complaints. In the postdecree and impact stages, these same measures are then used as independent variables to account for variability in the duration of suits across counties, modifications to ordered remedies, and changes in local spending for jails. Part of the point Welsh successfully makes is that outcomes at one stage influence events, decisions, and outcomes at succeeding stages. This is not to suggest that the later stages are fully predetermined but that earlier factors do have rippling effects in the reform process and that appreciating this will ultimately enhance the prospects of experiencing meaningful change.

For anyone interested in jails or public law litigation, *Counties in Court* is a notable addition to the literature and will serve as a useful source for investigations of institutional reform in the future. Welsh's style is generally easy to follow, and he does a yeoman job of piecing together divergent data sources to provide a solid, coherent picture of jail reform litigation as it has manifested itself in California counties. The case studies are interesting and Welsh is to be commended for not becoming overly bogged down in details. Statistical discourses are kept to a bare minimum, a technique that some might consider to be a plus. A complete and up-to-date set of references is included.

Closed Doors, Opportunities Lost: The Continuing Costs of Housing Discrimination. By John Yinger. New York: Russell Sage Foundation, 1995. 452p. $29.95.

Samuel L. Myers, Jr., *University of Minnesota*

For more than a decade, federal lending officials and the top brass at insurance and housing regulatory agencies denied the existence of illegal racial and ethnic discrimination in housing markets. They denied that the lenders they regulated engaged in impermissible discrimination in the granting of mortgage loans to African Americans and Latinos. They denied that real estate brokers or insurance agents engaged in federally banned redlining practices. From the election of Ronald Reagan in 1980 to the appearance of the disaggregated Home Mortgage Disclosure Act data in 1990, there were few government voices heard claiming that discrimination in housing markets existed.

With the appearance of the Urban Institute's 1991 housing audit studies and of the Boston Federal Reserve's controversial 1992 study of discrimination in mortgage lending, this all changed. Both presented clear and convincing evidence of continuing discrimination in housing and mortgage lending. For a brief period, the media vigorously reported the apparently astonishing findings of widespread and unchecked racial and ethnic discrimination in mortgage lending and in access to home purchase and rental opportunities.

John Yinger's *Closed Doors, Opportunities Lost* presents the best and most comprehensive evaluation of the scholarly and anecdotal evidence for the existence and persistence of racial and ethnic bias in housing markets in the United States. It reviews the causes and consequences of housing discrimination and examines the range of public policies available for redressing that discrimination. In between, the book explores the contemporary debates over spatial mismatches of jobs and residences, linkages between school segregation and housing access, measurement and detection of discrimination through audits, the limitations of the Home Mortgage Disclosure Act, and practically every methodological issue that has ever been raised in the housing discrimination literature.

The book begins by thoroughly defining and discussing the elusive concepts of race and ethnicity in a profound and appealing way. The intention and impacts of such an approach are clear: to reinforce Yinger's distancing of the book from contemporary scholarship that denies the role of discrimination and racism in creating social, economic, and political inequalities. The aim is to quickly establish the *social* and largely unstable nature of "race," in much the same way that Michael Howard Omi and Winant (1994) suggest in *Racial Formation in the United States.* Ethnic studies scholars will feel less than satisfied by the heavy reliance on several essays from *Science* and the *New York Times* to lay claim to these important conclusions. Omissions of prominent works of Ivan Van Sertima (e.g., *African Presence in Early Europe,* 1985) and Cheikh Anta Diop (e.g., *Civilization or Barbarism,* 1991) sadly leave underdeveloped this introductory section that is central to the thesis of the book. Nevertheless, it is noteworthy that the author appreciates the value of linking the analysis of racial and ethnic discrimination in housing to a very clear and unambiguous social definition of race and ethnicity. In so doing, Yinger argues against the hypothesis that there is something *wrong* with racial and ethnic minorities that contributes to their troubles in finding and owning homes.

Yinger then documents the extent of discrimination in housing markets, examining the evidence from fair housing audits and from the Home Mortgage Disclosure Act data. This is followed by a potentially explosive discussion of the costs to blacks and Hispanics of housing discrimination. Yinger calculates that "the cost imposed on blacks and Hispanics by current housing discrimination comes to $4.1 billion a year" (p. 103). Although the discussion is submerged in such arcane economic terminology as "starting surplus" and "willingness to pay," the meat of it will not be lost on attorneys litigating cases on behalf of plaintiffs in housing discrimination suits or minority reparations initiatives. If these costs are the appropriate economic measures of the illegal housing discrimination—in violation of state and federal statutes—then somebody is going to be very upset when they have to pay up. Of course, there are other costs outlined by Yinger, such as those associated with depressed employment opportunities and reduced educational access. But it is the $4.1 billion figure that stands out. Before long, some aggrieved party is going to say, "I want my $4.1 billion" and will point with authority to Yinger's seminal analysis.

That is why Yinger's introductory analysis of race and ethnicity is so important for understanding this work. Ultimately, as he argues in the concluding sections on policy options, any antidiscrimination effort worth undertaking must attack the core or foundation upon which racial and ethnic prejudice rests. Along with the standard admonishments of better enforcement of current laws, Yinger calls for revamped public rhetoric and acknowledgment and condemnation of racial and ethnic discrimination. "During the Reagan/Bush years," he says, "the public rhetoric was mixed . . . they gave comfort to people who want to discriminate and heightened the prejudicial attitudes that lead many blacks and whites to prefer not to live with each other" (p. 218). In essence, Yinger sees the problem of racism and ethnic bias as a social phenomenon influenced in substantial ways by political rhetoric.

Because he believes the "mixed signals" of political leaders and government officials of the Reagan-Bush era contributed to the current crisis, Yinger includes public statements denouncing racism in his list of policy prescriptions for reversing the crisis. He says, "The president and other national leaders could have a significant positive impact on race relations in this country with a regular series of strong public statements against racial and ethnic prejudice and discrimination" (p. 218). He concludes: "The national desperately needs a fair, nondiscriminatory program that attacks current discrimination, closes the disparities that hold back so many of our black and Hispanic citizens, and promotes individual and community responsibility" (p. 219).

It is unlikely, however, that the architects of the California Civil Rights Initiative—a massive anti-affirmative action undertaking—or the author of the Louisiana governor's executive order barring affirmative action in public employment, contracting and procurement, and public education would disagree with a proposal that is "fair" and "nondiscriminatory." The question is how political rhetoric can help. Could it possibly hurt, given the play on words one finds in current anti-affirmative action campaigns? Yinger recognizes the deleterious effects of negative political rhetoric on housing market discrimination. But can the reversal come about merely by public rhetoric that denounces racism? Indeed, since many of the political opponents of antidiscrimination initiatives have embraced the language he proses, one wonders whether political rhetoric—once its harm has been done—can work in reverse. Is there symmetry in the bad and good public rhetoric underlying racism?

Yinger makes a persuasive case that housing discrimination continues to persist and that the political rhetoric of the Reagan-Bush era gave legitimacy to the lackluster enforcement efforts of the 1980s. Yet the case still must be made for reversing this trend by adopting vigorous political rhetoric alongside such other noteworthy efforts as strict enforcement other existing laws or better support for schools and jobs. Ultimately, one must question whether the political rhetoric that seems to contribute to racial, social, and economic inequality is the *cause* or the *reflection* of the racially hostile marketplace the book documents.

Comparative Politics

Soldiers, Civilians, and Democracy: Post-Franco Spain in Comparative Perspective. By Felipe Agüero. Baltimore: Johns Hopkins University Press, 1995. 316p. $48.00.

Donald Share, *University of Puget Sound*

Spain's remarkable transition from authoritarianism to democracy between 1975 and 1978 is often cited as the paragon of peaceful and relatively rapid regime transition. However, the failed coup attempt of February 23, 1981, in which Spain's parliament and government were held hostage by disgruntled military officers, highlights the crucial role of the military in even the most successful transitions to democracy. Felipe Agüero's outstanding book provides a much needed framework for comparing the role of the military in transitions to democracy.

This book covers little new ground in its treatment of Spain's transition to democracy, though it does provide a very useful description of the Spanish armed forces during the democratization process. Instead, Agüero's most important and original contribution is his development of an entirely compelling comparative framework with which he attempts to explain the role of the military during regime change. In the tradition of the best comparative research methodology, Agüero deftly uses the Spanish case for both inductive and illustrative purposes.

Agüero correctly notes that the literature on democratization is now replete with comparative studies of economic, international systemic, political institutional, and leadership causes of democratization. Agüero's work is the first to focus on the crucial role of the military. Indeed, even after transitions to democracy in Southern Europe and Latin America, the military remained a (arguably *the*) major threat to the new democratic order. Agüero's main research task is to identify what factors allowed democratic reformers to resist military pressure both during and after the transitions to democracy. Agüero wants to know "how [reformers] empower themselves to lead the military to tolerate the establishment of a political regime it initially did not favor" (p. 6). The existing literature on democratization tends to reduce the military to a purely reactive institution, responding to changes in society at large. However, Agüero reminds us that the military in the cases he considers is often the institution most insulated from society. In order to better answer his research question, Agüero advocates a look at some variables that have been largely passed over in the existing literature.

First, he urges consideration of the role of the military in the outgoing authoritarian regime. The Spanish and Portuguese authoritarian regimes (*civilianized regimes*), in contrast to their Latin American counterparts (*militarized regimes*), did not place the military at center stage. Latin American militaries were thus better situated to control the nature of the transition to democracy. Second, Agüero calls attention to the "path" of the transition, distinguishing between evolutionary transitions from authoritarian rule and cases of collapse of the authoritarian regime. Third, Agüero also focuses on the role played by internal divisions within the military. Spain's military was divided between the majority of Franquist loyalists and a small but visible cohort of democratic reformers. Similar divisions were less visible in Agüero's Latin American cases, in part due to the need to close ranks against attempts to prosecute members of the military for human rights abuses. The presence of such intramural divisions greatly facilitated the initiation and consolidation of the Spanish transition, while the absence of such schisms constrained democratic consolidation in Latin America. Agüero also skillfully presents (chap. 6) a set of variables determining the success of democratization and military reform in the posttransition period, including the strength and coherence of the government (the absence of which largely explained the attempted coup by the Spanish armed forces in 1981), the ability of the government to maintain social order, and the timing of military reforms.

Spain's experience is compared with southern European and Latin American cases throughout the book; but for general readers interested mainly in the comparative analysis of militaries in democratic transitions, chapter 7 contains an excellent application of Agüero's model to the Greek, Portuguese, and Latin American cases.

This carefully crafted work has few shortcomings; but as any original and provocative work, it does raise questions. In his treatment of the Spanish military Agüero correctly contends that despite the historical antagonism between the Spanish Socialist Workers Party (PSOE) and the military, the PSOE governments after 1982 were in a stronger position to impose serious reforms of the Spanish military than the Socialists' conservative predecessors. In light of recent allegations that top Socialist officials may have coordinated a death squad-like operation against suspected Basque terrorists, one wonders whether the Socialists were indeed as free from military pressure as Agüero suggests. If Socialist complicity in these death squads is proven, was it an attempt by the PSOE government to allay military concerns about Basque terrorism?

The most serious question raised by this book is why Agüero chose his case studies entirely from southern Europe and Latin America, following in the footsteps of many works in the 1970s and 1980s. During those decades, however, the justification for such selection of case studies was that most cases of democratization were limited to these two regions. The 1990s, however, have seen the deepening of what Samuel Huntington has called the "Third Wave" of democratization. One wonders why Agüero did not try to apply his model, at least briefly and speculatively, to the more recent transitions in Eastern Europe, Asia, and South Africa. Agüero gives us a smattering of such broader comparative analysis in a couple of points in the book but never takes the analysis of these cases any farther. Indeed, including cases from Eastern Europe and South Africa would have made Spain seem far less unique, at least in terms of being "the only state among those considered . . . which is made up of distinct nationalities" (p. 137).

The mechanics of this book are first-rate. Agüero writes clearly and manages to avoid the jargon so common in political science works. The book is methodically researched and brilliantly organized. The only technical flaw is the curious absence of a bibliography on democratization and the military, but this lack is partly offset by the extensive documentation in the endnotes. This book not only stands out as a fine contribution to the literature on democratic transitions, but it should also be recognized as a fine example of thoughtfully conceived and carefully structured comparative analysis.

Feminists, Islam, and Nation: Gender and the Making of Modern Egypt. By Margot Badran. Princeton: Princeton University Press, 1995. 352p. $35.00.

Women and the Political Process in Twentieth-Century Iran. By Parvin Paidar. Cambridge and New York: Cambridge University Press, 1995. 401p. $59.95.

Masoud Kazemzadeh, *University of Alabama at Birmingham*

From the eighteenth century until the 1960s, the study of Middle Eastern polities had been dominated by the Orientalist paradigm. According to this paradigm, Western social scientific concepts of class, nationalism, and gender are as irrelevant to an understanding of the Middle East as are Western ideals of equality, rule of law, democracy, liberalism, and socialism, wherever Islam determines the behaviors of rulers and ruled alike.

In the late 1950s and early 1960s, the modernization paradigm replaced the hitherto-unchallenged Orientalist paradigm. The persistence of authoritarian forms of governments was explained by a new breed of modernization scholars utilizing political culture explanations to argue that the lack of a civic culture—caused primarily by Islam in the Middle East—accounts for political backwardness despite obvious economic and social progress. Modernization theorists discarded all but two concepts from the Orientalist paradigm: an essentialist conception of Islam, in which Islam is viewed as a set of unchanging dogmas and principles, and the concept of national character.

In the early 1970s, neo-Marxist and dependency school scholars specializing in the Middle East employed concepts of class and imperialism in their analyses. According to these scholars, imperialism and colonialism were the causes of backwardness.

The emergence in the late 1970s and early 1980s of Islamic fundamentalist movements and regimes—that appeared to be atavistic and intent on proving true the worst stereotypes of Muslims and Islam—gave rise to a new paradigm that has been called neo-Orientalism. The neo-Orientalists share with the Orientalists the same essentialist view of Islam. Moreover, this unchanging and ahistorical Islam is regarded as an independent variable determining behaviors—political or otherwise—in these polities.

The two books under review are among an emerging body of scholarship in Middle East studies that utilize Western social scientific concepts of gender, class, and ideology to challenge the aforementioned paradigms. They criticize Orientalist, modernization, and neo-Orientalist scholars for their essentialist notion of Islam. Instead, they argue that there are numerous and conflicting Islamic groups and states, and, therefore, that the view of Islam as coherent and homogeneous is false. They criticize the dependency and neo-Marxist scholarship for ignoring the saliency, if not the centrality, of gender and patriarchy in the analysis of Middle Eastern polities.

Paidar is explicit in her challenge to the above paradigms and discusses them in detail in the introductory and concluding chapters. Moreover, she situates her analytical framework within feminism and Middle East women's studies. Badran's work, which is thoroughly atheoretical, presents an implicit challenge. For Paidar and Badran, feminists, women, and issues of gender have been at the center of political change in Iran and Egypt, respectively; however, neither scholar tells us what criteria a polity should meet for making gender analysis analytically fruitful. Both define "feminism" and "feminist" so broadly—as encompassing any idea, person, or movement that seeks to improve the status and situation of women—that even Phyllis Schlafly would qualify as feminist.

Paidar utilizes the concept of "discourse" to analyze the ways in which "the state, political parties, social movements and civil society communicate, relate to each other, negotiate power and control, and take action" (p. 23). Although Paidar mentions classes, especially in earlier chapters, the various discourses are primarily analyzed as debates disembodied from social classes. Badran, on the contrary, is careful to mention the class backgrounds of the various actors as well as the socioeconomic context of the debates. Badran's work, however, neither draws on any theoretical guides nor makes any theoretical conclusions.

Paidar's goals are twofold: to challenge the dominant studies of Iran that have marginalized women and, relatedly, to present an analysis that situates women at the center of Iranian politics. Paidar identifies three major discourses in twentieth-century Iran: modernity, revolution, and Islamization. In all these discourses women's role and status were at the center of debate.

Paidar argues that the discourse of modernity, which equated modernity with Western societies, was dominant from 1905 until the mid-1970s. In the period from 1905 to 1910, women's emancipation was regarded as necessary as well as a signpost of modernization and progress. Iran could not achieve modernity, many believed, unless women were allowed to be educated, to be employed, and to become full citizens. In the 1920s to 1940s, women's dress and social activities came to be regarded as signs of the socioeconomic modernity that the state was promoting. In the early 1950s, women's issues, such as female enfranchisement, became salient and hotly debated in the Parliament. In the period between the mid-1950s and the early 1970s, the Shah imposed reforms that had long been feminists' demands, such as women's right to vote and be elected, as well as reform of the sensitive personal status code (e.g., minimum age of marriage, divorce, polygamy, custody of children, inheritance), which restricted male patriarchal privileges that the *Shariah* had granted men.

The period between 1977 and 1981, according to Paidar, encompasses the discourse of revolution. In the discourse of revolution, gender relations were revolutionized in Iran. Masses of women participated in rallies and strikes and were martyred during the revolutionary process. Afterward, women participated in struggles both opposing and supporting Khomeini. The discourse of the revolution revolved around the debate over cultural authenticity versus imperialist culture. The role of women loomed large in the discussion of what constituted a true indigenous model versus imported alien models.

For Paidar, the discourse of Islamization started sometime after Khomeini's ascendancy and continues to this day. Khomeini suspended laws relating to personal status that had been promulgated by the Shah: the age of marriage for girls was returned to nine; men's absolute right to divorce was reinstated; custody of children was given automatically to the father or paternal grandfather; and restrictions on polygamy were lifted. However, the Islamic Republic did not rescind female franchise even though Khomeini had opposed it in 1962. Instead, the ruling elite mobilized its female supporters against secular women. In the early 1990s, however, the regime reversed itself and reinstituted many of the personal status laws that the Shah had promulgated.

Paidar both succeeds and fails in situating gender at the center of Iranian politics. Her evidence certainly indicates that gender was one of the most salient issues in twentieth-century Iranian politics. Her arguments, however, reinstate the saliency of Islam in determining the importance of gender, which was a basic tenet of the Orientalist, political culture, and neo-Orientalist paradigms. According to Paidar, "The rapid Islamisation of women's position pointed once again to the centrality of gender relations in the political ideology of Islam.... Ayatollah Khomeini was adamant that without the Islamic family and women's hejab there could be no Islamic society" (p. 232). Orientalists, political culture scholars, and neo-Orientalists would concur wholeheartedly.

Although Paidar begins and ends her book by castigating those who hold an essentialist view of Islam and indeed reiterates this several times in her discussion, her analysis fails to distinguish between fundamentalist and nonfundamentalist clerics and groups. Patriarchy and Islam are treated as one and the same, and the policies of the fundamentalists are treated as though all Islamic groups share that interpretation of Islam. The term *fundamentalist* is not used even once in this lengthy book, nor is any other term used to connote a major difference between Khomeini and his supporters, on the one hand, and other Islamic groups, on the other. Terms such as *theocracy*, *theocratic state*, *clerical rule*, *the Islamic regime*, and *the Islamic state* are used interchangeably to refer to the rule of Khomeini and his followers. In addition, terms such as *the clergy*, *the hardline clergy*, and *the Islamic leadership* are used to refer to Khomeini and his clerical supporters. Referring to Khomeini's policies, Paidar consistently uses the term *Islamisation*. For Paidar, it seems, only one Islam exists and the various Islamic groups only differ as to the pace and mode of enforcing the unambiguous Islamic rules.

Badran's aim is to tell the story of the Egyptian women who created the modern Egyptian polity. Her work spans the time period between the late nineteenth century and the mid-1950s. Relying on memoirs, women's journals established by women, and extensive personal interviews she conducted, Badran weaves together a lucid and coherent account of how a handful of upper-class and middle-class women influenced modern Egypt. The book reads like an encyclopedia of biographies of female firsts. The struggles and the family backgrounds of the first female graduates of elementary, primary, middle, and high schools are described. The stories of the first women admitted to various disciplines at universities are beautifully told. The readers are made to appreciate the struggles of the first female social worker, teacher, journalist, lawyer, pilot, and actress and those who publicly played tennis.

There are four leitmotifs in Badran's story. First, Egyptian feminist women were central in the making of modern

Egypt. Second, feminism in Egypt was indigenous and not imported from the West. Third, Egyptian feminists were at the forefront of nationalist struggles against the British colonialists. Fourth, Egyptian feminists advocated reforms that were in accord with Islamic modernist readings of Islam.

Constant exhortations notwithstanding, Badran's own evidence seems to refute her first and second claims and only support the third and fourth claims. Although Egyptian feminists participated in all the important struggles of twentieth-century Egypt, none of their demands was implemented, with the exception of raising the minimum age of marriage for females. Moreover, since the mid-1970s, an increasingly large number of Egyptian women have embraced fundamentalism, which is the most reactionary and patriarchal form of Islam. These two observations seem to indicate that the feminist movement in Egypt has had neither a major influence on the laws of the land nor a major impact on its women, therefore, casting doubt on Badran's first claim. On the second claim, Badran's own extensive quotations of Egyptian feminists indicate that they constantly and explicitly used the position of women in Western countries as models and examples to be followed (pp. 182, 188, 240).

In her eagerness to provide a non-essentialist view of Islam, Badran presents the Egyptian feminists' modernist reading of Islam to be just as valid as the traditional and fundamentalist views. The Egyptian feminists, for example, have argued that raising the minimum age of marriage to 16 for females is "an enlightened interpretation of the Islamic shar'iah" (p. 242). The fact of the matter is that on this issue the fundamentalist and traditionalist position, which regards nine as the minimum age of marriage for females, is the only interpretation of the Shariah, enlightened or otherwise. The Prophet Mohammad consummated his marriage to Ayeshah when she was nine years old. To raise the age of marriage to anything above nine would locate the Prophet's marriage outside Islamic law and therefore make it a sin.

The fact that fundamentalists do not demand the reinstitution of slavery, despite being allowed by the Shariah and the fact that the Prophet himself owned both slaves and concubines, indicates that even the fundamentalists do not call for the complete implementation of the Shariah. To oppose the Orientalists' essentialist conception of Islam is to recognize the differences among the various Islamist groups who have used Islam to justify slavery, precapitalist mercantile bazaar economy, capitalism, socialism, antifeminism, and feminism; however, one cannot extrapolate from this that any reading of Islam is historically accurate. In other words, the analytically significant point is to recognize that the principles of Islam do not determine the behaviors of Islamist groups, but rather that various Islamic groups pick and choose, as well as interpret Islam to justify their own material interests, which might be related to class, gender, or ethnicity.

In addition, there are two minor errors in Badran's book. On page 286, note 49, Badran asserts: "In Shi'i Islamic jurisprudence women and men inherit equally"; this is simply not true. On page 312, note 45, the U.S. president is rendered "Theodore Roosevelt"; in 1938, the U.S. president was Franklin Roosevelt.

These two books will not generate much scholarly debate because neither offers a novel analysis demanding either challenges or defenses. However, both books will be cited by scholars in the field because each presents the most comprehensive historical description of women's and feminist movements in each respective country. This alone is a major achievement.

The Politics of Water: Urban Protest, Gender, and Power in Monterrey, Mexico. By Vivienne Bennett. Pittsburgh: University of Pittsburgh Press, 1995. 195p. $49.95.

Helen Ingram, *University of California at Irvine*

Useful and wide ranging, this case study manages to address a number of topics. Vivienne Bennett uses municipal water supply policy in Monterrey as a window through which to view unbalanced urban development, power structure relationships in Mexico, social protest movements in the developing world, and the advisability of privatization of public utilities. Along with the wealth of information provided, the broad bibliography leads the reader in many interesting directions.

It is ironic that a city whose economic progress was an engine to the Mexican miracle was unable to serve its citizens with a dependable supply of water for decades. Vivienne Bennett unravels the puzzle by tracing public spending. She explains the patterns of public appropriations that provided generous funding for Mexico D.F. and a few other cities while skimping in the provisions for Monterrey. Lack of balance between urban and rural public investment, with little going to agriculture apart from that related to foreign trade, was also responsible in part for the flood of immigrants to Monterrey and other cities. The water needs of these newcomers were far greater than the physical capacity of infrastructure to serve.

A powerful consortium of private business owners, the Grupo de Monterrey, was also close to the heart of the water problem. Fiercely opposed to PRI, the dominant political party since the Mexican Revolution, the Grupo de Monterrey was at odds with the central administration. When the Grupo de Monterrey dominated the Monterrey water agency, the water agency could depend on little outside help. Because most businesses depended on unregulated private wells, industry could continue to have water while their workers, who were dependent upon municipal supply, went without.

Water is the kind of issue that can mobilize even the most apathetic public. Vivienne Bennett tells the fascinating story of low-income housewives who took to the streets, blocked traffic, and kidnaped and held hostage water agency personnel. Practically leaderless, they were successful in meeting their objectives, at least in part because leaders could not be coopted or removed. Oddly, poor women managed to force action at the same time as the Grupo de Monterrey could not. However, while such movements can effect changes in the operation of some government functions, their broader ability to transform society is questionable. Once their demands for water are satisfied, protest and mobilization disappear.

The strong president style of Mexican government played a role in both causing and curing Monterrey's water crisis. The height of the problem occurred at a time when President Echeverría and the Grupo de Monterrey were at loggerheads. Mayors are appointed by the president, and when lame duck mayors are in office, little help from the central government can be expected. Such a problem delayed action by the central administration for almost two years until President López Portillo, who had good relations with the Grupo de Monterrey, was able to appoint his own person as mayor.

Using the example of Monterrey, Vivienne Bennett questions whether privatization is likely to provide the overall solution to poor water management that some enthusiasts claim. The problem with inadequate infrastructure began during a time when a Canadian company had a franchise to provide water to Monterrey. While poor people pay a larger portion of their income than do the rich for water services and although the poor are able to support infrastructure projects, the key is that fees must be collected. People are reluctant to pay for water services they do not receive. A vicious cycle develops in which repairs cannot be made for lack of resources, and rates cannot provide resources because customers refuse to pay.

Valuable for what it covers, this book has some significant failings. It gleans nothing from and contributes little to natural resource or other policy studies. The book treats water needs as if they were static and water supply as if it were simply a matter of building infrastructure. The culture of technological bureaucracy that is so important to understanding how problems are framed and alternatives chosen is completely ignored. Even the politics of reorganization that is fundamental to Mexican bureaucracy goes without explanation. However, there are few issues more important or more frustrating to Mexico than water, and this book contributes a good deal to understanding why.

State, Capitalism, and Democracy in Latin America. By Atilio A. Borón. Boulder, CO: Lynne Rienner, 1995. 262p. $49.95.

Ronald H. Chilcote, *University of California, Riverside*

The major theme in this important book revolves around the possibilities and limitations of democratic capitalism in Latin America. This problematic evolves rather deliberately through a retrospective analysis of questions on the tensions and contradictions between capitalism and democracy, the role of the state in modern capitalist society, the myth and reality of neoliberal doctrine and discourse, the autonomy of the state, social reform and democratic consolidation and persistence, and the crisis of Marxist political theory and its prospects for renewal. These questions are explored at a general level and are then related to the particular Latin American situation.

Borón begins with some guidelines for understanding the complex and obscure problem of the contradictory relationship between capitalism as a mode of production and democracy as a political regime. For Latin America the relevance of this problem became particularly clear beginning in the mid-1960s when military dictatorships proved necessary in assuring the deepening of capitalist development, thereby exposing the myth, promoted especially among mainstream North American social scientists, that through capitalist development the region's chronic authoritarianism could be eradicated and bourgeois democracy secured. He reveals different perceptions between classical and contemporary theorists of democracy as the earlier revolutionary and liberating substance of democratic ideas has gradually given way to a convergence of democracy and capitalism in the twentieth century leading to varied outcomes: "When bourgeois hegemony succeeded in introducing into the consciousness of the subordinated classes the ideological justification of its class domination, the democratic state could coexist" without force; yet when "bourgeois hegemony was not achieved, the faulty constitution of democratic capitalism rapidly gave way to fascism or dictatorship" (p. 8).

Given this paradox, Borón suggests that the historical relationship between capitalism and democracy reveals the need to separate the direct producers and owners of the means of production, on the one hand, and state and civil society, on the other, as the means of ensuring a proletariat able to sell its labor power. He shows that while bourgeois revolutions established a liberal state, they did not establish bourgeois democracy: "It was the prolonged contestation and rebellion of the laboring classes . . . that democratized the capitalist state" (p. 11). Yet reversion to despotism is always possible in such situations, and in backward areas capitalist penetration failed to reproduce the development of early bourgeois countries. Consequently, he argues, the democratization of the capitalist state in Latin America will be "the people's victory," and rather than serving to accommodate "the authoritarian edges of bourgeois domination . . . it should accelerate toward superior forms of political organization, the integral and substantive democracy of socialism" (p. 27).

Borón also shows that liberal conceptions of the market misrepresent and distort the role of the state in shaping and reproducing capitalism. The state not only ensured hegemony of ruling class interests, but it also shaped a new model of accumulation and capitalist development in which it actively intervened to mitigate periodical crises inherent in capitalism. Therefore, the proposition of neoliberals that one must choose between market and the state is false, for the dilemma is a choice between the market and democracy. For neoliberals, "democracy is the real enemy, lying at the bottom of the antistate criticism of neoliberalism" (p. 64).

A new conception of democracy appears in the discourse of Tocqueville. On the one hand, capitalist society creates the conditions that lead to a burdensome and oppressive state; on the other, social substance and political form constitute "the double character of democracy." Social democracy and political democracy are inseparable, yet neoliberals and capitalism today, according to Borón, do not embrace the proposal of Tocqueville that trust be placed in civil society and in "a full recovering of politics, the reactivation of the citizenry, and the reanimation of public life" (p. 91).

Delving into the mystery of how political science obscured the concept of state in a turn to the political system, Borón examines the intellectual poverty of Western political science and delves into various approaches between the state and civil society that emerge in Marxism in its critique of both representative and exceptional forms. He exposes the weaknesses of instrumentalism, preferring instead a structural foundation and an emphasis on the relative autonomy of the state. Drawing heavily upon Antonio Gramsci and Nicos Poulantzas, the relative autonomy of the state becomes structural and "built into the very foundation of this mode of production and the laws of motion" (p. 126).

Neoconservative pragmatism and the neoliberal position call for the destruction of the state, a view Borón criticizes as dogmatic and mistaken. He doubts that moderate reforms can overcome the inefficient and bankrupt state whose services are insufficent and whose bureaucracy behaves oppressively. It is for progressive Latin American thought, he believes, to confront the issue of state reform with seriousness and responsibility, but no particular approach to this problem is offered and tentative conclusions follow: contemporary Latin American democracies have somehow resisted difficult conditions imposed upon them by economic crisis, inflation, and poverty, yet democracy may well fade away because the interests of the wealthy really count in the face of a decaying political scene characterized by the failure of parties and congresses to be representative and the corruption of the state apparatuses. The collapse of the first wave of socialist revolutions does not imply the ultimate disintegration of socialism; more likely a creative and profound presence of Marxism will lead to immense possibilities in the contemporary world.

Atilio Borón, who holds the chair in political theory at the University of Buenos Aires, provides a fresh and engaging synthesis of significant themes relevant to political science and political economy. The coverage of concepts, issues, and debates that have caught academic attention over the past two decades is presented in a way that motivates us to confront the challenge of analyzing the contradictions of capitalism and democracy and the need to find a progressive road toward democratic consolidation on the periphery.

Political Liberalization and Democratization in the Arab World, volume 1: Theoretical Perspectives. Edited by Rex Brynen, Bahgat Korany, and Paul Noble. Boulder, CO: Lynne Reinner, 1995. 350p. $23.95 paper.

Augustus Richard Norton, *Boston University*

Is the title *Political Liberalization and Democratization in the Arab World* sardonic? Given the recent democratization reverses in Algeria, Egypt, Tunisia, and Yemen, one might think so; but in fact the editors are not attempting humor. To the contrary, their intention is to address those themes that are essential for any serious study of political liberalization and democratization in the Arab world. No scholar or expert familiar with the Arab world would predict the inevitable triumph of democracy there, any more than one would do so vis-à-vis Africa or East Asia. Nonetheless, the 1980s and 1990s have been a remarkably tumultuous period in Arab politics, not because governments have frequently changed hands—they have not—but because the nexus of state and society is increasingly a troubled one. As a result, since the mid-1980s politics within the Arab world have reflected numerous attempts to reconsolidate the power of the state, including the cautious easing of state controls on political life.

In their excellent introduction, the editors usefully distinguish between democratization and political liberalization. Even by their minimalist definition, wherein democratization refers to an expansion of participation that allows citizens a real voice in controlling public policy, examples of democratization are arguably limited to the examples of Jordan, Kuwait, and Lebanon. Yet, defined as the expansion of public space through which individuals gain protection for civil and political rights, liberalization is far more common, especially to the benefit of those segments of society prone to support rather than oppose the state.

The significant growth of civil society is a reflection of the sporadic lifting of government controls on associational life, as Saad Eddin Ibrahim shows (pp. 29–57). Nonetheless, as Mervat Hatem persuasively demonstrates, the growth of civil society has not been gender-neutral in its effects (pp. 187–208). As women's associations have been propelled into the public sphere, gender issues have often been sacrificed by governments in order to reconsolidate their control vis-à-vis the Islamists in particular. Equally important, much of the writing on civil society, including Ibrahim's, devotes scant attention to informal associational life in the urban lower class where the bonds of associability are more likely to be personal and reciprocal than contractual. This is a significant lacuna, since informal association in the vast urbanscapes of the Arab world is extensive as well as crucial for understanding the consequences of political liberalization or democratization.

Arab rulers have sometimes teased their subjects with the promise of democracy, but, even when opposition parties are tolerated, orchestrated government electoral victories are the norm. Other than the abortive Algerian experiment with democracy from 1989 to 1992—the exception that proves the rule—no Arab state has been willing to fully embrace the system of bounded uncertainty known as "democracy." Thus, in the Egyptian parliamentary elections of 1995 fewer than 3% of the seats were won by the opposition. This was not an astounding display of popular support for the government-controlled National Democratic party but rather a product of egregious electoral fraud and extensive harassment and widespread arrests of the opposition, including several candidates. Inevitably, any serious consideration of political liberalization and democratization confronts the modern persistence of authoritarian rule in the Arab state system.

The Arab governments, most of which emerged from Western domination only in the last half-century, have stilled opposition voices while sustaining political quiescence by promising social justice and economic development. This legitimacy formula is now under severe challenge and the dreams that inspired two generations of Arabs to lend support to autocratic regimes have faded. There is no disagreement about the problems impinging on the regimes, many of which are familiar to any student of the developing world: explosive rates of urbanization, unemployment, declining public services, bloated and inefficient public sectors, and reduced state resources. Despite this formidable array of problems, the regimes have proven to be remarkably durable and highly resistant to pressures for profound political and economic reform. Instead, focusing on the non-oil rich states, Daniel Brumberg argues that the rulers have pursued survival strategies intended to co-opt business and professional elites while avoiding extensive reforms (pp. 229–59). In general, these strategies have failed to address and have even exacerbated underlying economic problems, so they only forestall the need for more profound change. Brumberg speculates that this change may initially encompass political liberalization intended to foster a dependent capitalist class following the model of the "Asian tigers" (p. 253). Brumberg may concede more creative imagination to the governments than is merited, and certainly more potential support. As Giacomo Luciani notes, the governments are "utterly discredited" and the only real option for reconsolidating authoritarian control may be through an alliance with the Islamist opposition, or parts of it (pp. 224–5). Given the populist character of Islamist ideology and the reluctant but inevitable implementation of economic restructuring that jeopardizes state subsides and public sector jobs, this would hardly be a divine marriage. Under these circumstances, a democratic bargain may be much more palatable, particularly if the terms of participation ensure that the Islamists will be limited to minoritarian representation.

There is no support in this volume for the sort of essentialist assertions proffered by prominent anthropologists (e.g., Ernest Gellner), historians (e.g., Elie Kedourie), and a variety of polemicists to explain the absence of democracy in the Arab world. As thoughtful and complementary chapters by Michael Hudson and Lisa Anderson demonstrate, an analytically rigorous approach to the question of political culture yields a different landscape than that depicted by influential denizens of Orientalism (pp.

61–92). Islamic political thought, ever the independent variable in the writings of Orientialists, appears in this volume as adaptive, pluralistic, and contentious. Textualist interpretations of Islamic political thought are an inadequate guide to the political attitudes and the behavior of Muslims whose habituation to authoritarianism has much more to do with the policies and controls implemented by their governments than the verses of the Holy Quran, as Gudrun Krämer illustrates in an impressive analysis of Islamic thought on pluralism, which she juxtaposes to case examples of Tunisia and Egypt, where the Islamists have shown a striking pragmatism, confounding the textual evidence (pp. 113–28). Janine Clark's welcome contribution on Islamic health clinics in Cairo makes much the same point as Krämer (pp. 167–86).

The debate between the fourteen contributors in this important volume is whether the locus of political change and reform can emanate from the deeply entrenched, fortified, and rent-dependent regimes that characterize the Arab world or from societies where the ideology of opposition is predominantly but not solely Islamist. If, on the one hand, the answer is that change may come only from above, then the focus of several contributors on the political economy of regimes and their regional and international environment is crucial. On the other hand, if one focuses on society, it is imperative to understand the limits and potential of civil society, as well as the substance and tone of political discourse on central questions such as democracy (and there is an interesting intellectual Arab discourse about democracy, as Salwa Ismail shows [pp. 93–111]). Regardless of perspective, there is no question that the program of democratization is only roughly related to the agenda of political liberalization and that political liberalization has been often implemented for very nondemocratic goals by the state and applauded and promoted by leading elements in civil society for precisely that reason.

As scholars focus on the debate, as well as the etiology of the problematic state-society nexus, serious and rigorous work will benefit from the insights compiled by Messrs. Brynen, Korany, and Noble. In contrast, the crude cultural analysis, which often informs even the work of leading comparativists, that objectifies Islam, ignores the role of external powers in giving shape to Arab political institutions, underestimates the significance of associational life outside of state control, and fails to understand the social reconstruction of "traditional" social formations, such as the tribe, has very little to contribute and deserves a decent burial. *Political Liberalization and Democratization* would make good reading at the wake.

SDP: The Birth, Life and Death of the Social Democratic Party. By Ivor Crewe and Anthony King. Oxford: Oxford University Press. 611p. $47.00.

David E. Butler, *Nuffield College*

This is a notable contribution to political science. The story of the birth, life, and death of a party that for a short time seemed likely to break the mold of British politics would always have been a worthwhile enterprise. But this book transcends mere narrative; its importance lies in the way it analyzes the essential elements of a political system that gave the new party its chance but that, in the end, frustrated its high ambitions.

The Social Democratic Party was formed in 1982. It won some notable by-elections and with its allies briefly topped both Conservative and Labour in the opinion polls. But by 1990 it was dead. The two authors, both close to the party but academically detached, decided from the outset that the party progress should be authoritatively chronicled. They have produced not just a highly readable story of success and failure but also a serious exploration of the structural constraints on realignment within a well-entrenched party system.

As with so much of recent British politics, the roots of the SDP lay in the European issue. When Edward Heath was driving the country into Europe in 1972, Roy Jenkins and his friends felt alienated by the Labour Party's hesitations. Although Harold Wilson's maneuvers and the 1975 referendum seemed to establish party acceptance of EEC membership, the Labour Party once again became anti-European after its loss of office in 1979. This was too much for Shirley Williams and David Owen and—together with Roy Jenkins, back from a stint as chairman of the European Commission—they formed the breakaway Social Democratic Party, which won the support of 26 sitting Labour MPs. In alliance with their Liberal friends the center grouping almost drove the Labour Party into third place in the 1983 election. They were a mere 2% behind in votes—but they got only 23 seats (6 SDP and 17 Liberal). The SDP struggled on, but a repeat disappointment in the 1987 election led to demands for a full merger with the Liberals. The leader, David Owen, would not cooperate and formed a continuing SDP—but that finally folded in 1990.

This story is full of hope and drama and disappointment. The reader is constantly challenged to ask whether things went wrong because of chance events or personal rivalries or basic elements of the British political system.

In the winter of 1982 everything seemed to be going the way of the new party. But then the Falklands War, coinciding with a moderate upsurge in prosperity, transformed the political scene. Mrs. Thatcher led the Conservatives back from the abyss of Crosby to their 1983 triumph. Roy Jenkins failed to emerge as a charismatic political leader and the relations between the SDP and the Liberals were at times conspicuously acrimonious. Many of the new recruits, drawn into the party by its freshness and freedom from vested interests, became disillusioned. The party's foothold never became a bridgehead.

Crewe and King write vividly and critically about each step in this saga. There were the agonizings over whether to desert the Labour Party; there were the early fights over party structure and the absurd dream of running the party through a collective leadership; there were the battles with the Liberals over the allocation of seats; there were the secret attempts to displace Roy Jenkins in the midst of the 1983 campaign; there was the overt rivalry between David and Owen throughout the 1987 election; and there was the hilarious fiasco of the "dead parrot" manifesto during the struggle to merge the two parties in 1988.

The authors explore the conflicting ambitions of the principal actors. Roy Jenkins and his colleagues were a notably attractive and intelligent group of politicians, struggling against the limits imposed by the electoral system, by parliamentary customs, by the fickle media, and by the obstinate habits of British voters. Roy Jenkins never realized his dream of becoming prime minister, but he left a real mark on British politics.

The authors explore in a fair-minded way what went wrong. Perhaps they are too critical in their obituary of the party—"a study in failure." Was it altogether a failure? The early days saw experiments in party structure that their rivals later copied—direct election of leaders and candidates, centralized party membership lists, more elaborate policy consultations, direct mail for fund-raising and opinion-sounding. More important, the SDP did turn the Labour Party around. Neil Kinnock learned from Labour's appalling performance in 1983 that the party had to move back to the center and recover the clothes that the SDP had stolen. If in the mid-1990s Labour is poised in a winning position, it is because its leaders belatedly followed the path set by the Gang of Four in 1982.

Poland's Permanent Revolution: People vs. Elites, 1956–1990. Edited by Jane Leftwich Curry and Luba Fajfer. Washington, D.C.: American University Press, 1996. 294p. $64.50 cloth, $27.50 paper.

Robert Zuzowski, *University of the Witwatersrand*

This is a splendid collection of articles, five of them, all dealing with Poland's political crises, beginning with the October 1956 crisis and culminating in the 1989 collapse of the communist regime. The volume opens with an introductory chapter. It sets the background for the political history of Poland after World War II and also examines the value of general models and theories of democratization. What follows is a shrewd analysis of crisis management in Poland: popular expectations versus the Polish political elites. This is perhaps the best book on Poland's politics published in recent years. While it is a collective volume, it almost reads as a text written by one author.

The volume incorporates previously censored archival information as well as information gathered from memoirs written recently by prominent Polish politicians. All the articles are well (sometimes even too well) documented, which makes it occasionally tedious reading. Although all the individual contributions are generally of high quality, they add little to our understanding of crisis management and the collapse of communism in Poland.

Despite the authors' thorough knowledge of Polish politics, a few statements made by them need comments. It is highly disputable, for example, whether "the notion of egalitarianism took roots in Poles' expectation and their rhetoric" (p. 8) only after the imposition of communism. In fact, this idea has a very long history in Poland going back several centuries to the times of noblemen's democracy. The same author maintains that "from the 1960s on they [the Poles] were allowed to leave Poland" (p. 9). This is inaccurate. In the 1960s, as a matter of fact, it was very difficult to leave the country, with the notable exception of 1968 when Jews could easily emigrate and were even "encouraged" to do so. It is wrong, too, to claim that in Poland "agriculture from 1956 on [was] virtually all in private hands" (p. 9), because when communism fell in the country only about 75% of all arable lands belonged to the private sector.

Several more shortcomings mar this otherwise very useful collection of essays. Jane Leftwich Curry identifies the white-collar class with the intelligentsia; however, though these two categories overlap, they are by no means identical. The notion of the intelligentsia is an exclusively Eastern European phenomenon, while the white-collar class is not. To describe the state and party bureaucracy as a kind of "entrepreneurial class" is, I think, to employ a misnomer. Nor is it true either that "Solidarity leader Adam Michnik" was a Communist Party member (p. 13). To maintain that in 1956 the Roman Catholic Church returned to power in Poland is unclear to say the least.

The first chapter, authored by Andrzej Korbonski, reassesses the significance of the 1956 political crisis. This is an extremely interesting essay and lucidly written. He concludes his assessment of it by saying that "the real meaning of October [1956 crisis] was neither a watershed nor a palace revolution" (p. 53). All information in this chapter is accurate, which cannot be said about the contents of most of the other chapters. For example, Sarah Meiklejohn Terry wrongly states that the Workers' Defense Committee (KOR) was born by mid-July 1976 (p. 133), whereas in fact it was established in late September 1976. She is wrong once again when saying that "many" of KOR's founders were former members of the Communist Party. The truth is that only four out of the fourteen KOR founders were former Communist Party members, and only six of KOR's total membership of thirty-eight belonged formerly to the Party.

Leftwich Curry writes that when martial law was introduced in Poland in December 1981 "hundreds" of Solidarity leaders were interned (p. 176). Actually, the number was much higher; it went into several thousands. She argues, wrongly again, that Solidarity was the "first independent trade union in a communist country" (p. 167). The first independent trade union in a communist country was the Seamen's and Deep-Sea Fishermen's Trade Union established in Poland on 4 November 1956 in Gdynia near Gdansk. When Karol Wojtyla was elected Pope he was not a bishop but an archbishop and a cardinal as well (p. 169). Colonel Kuklinski did not defect from Poland "in early 1981" (p. 189) but in November 1989. Cardinal Stefan Wyszynski did not head the Roman Catholic Church in Poland "since the [Second World] War," but since 1948 (p. 189).

Marjorie Castle, the author of the last chapter, claims that communists ruled Poland since 1947 (p. 211). On the book's back cover we read that the communist takeover occurred in Poland in 1948. The takeover actually took place, however, in 1944–45. And a Solidarity-allied prime minister was not sworn in to office in August 1989 (p. 211), but on 12 September (see p. 276). Communist rule in Poland lasted not for 41 years (p. 245), but for 45 years (from 1944 to 1989). The Polish rulers did not use violence in 1980 (p. 252); this happened a year later. Martial law was not put off since the spring of 1980 (p. 252), but since fall of the same year. KOR sought to help the workers not only in 1976 (p. 274), but throughout its entire existence, stretching from 1976 to 1981.

It is somewhat difficult to agree with the authors of the concluding chapter who argue implicitly that it was only under communist rule that "a Polish political discourse that was high on symbolism and low on pragmatism was established" (p. 258). This statement can easily be extended to refer to Polish politics since at least the late seventeenth century. To maintain "that little has really changed" in Poland since the collapse of communism (p. 258) is definitely wrong. Seven years of democracy, however fragile, is not tantamount to seven years of communism, nor does this amount to a "little" change. In fact, it is a fundamental change regardless of what will happen in Polish politics in the future.

It is difficult to do justice to this volume in the space alloted. Suffice it to say that the essays are for the most part of exceptional quality. This volume provides a real insight into political crisis management and the communist collapse. It is a timely publication, highly recommended to all students of communist and post-communist societies.

The book ends with a very pertinent question concerning political change to which the Poles—and not only them—have not yet found an answer: "The conundrum remains of how one moves [effectively] from one political and economic system to another." This question lies at the heart of political science. The person who first comes up with a satisfactory answer should therefore be awarded the equivalent of a Nobel Prize in science—that is, a Nobel Prize in political science.

State Building and Democracy in Southern Africa: Botswana, Zimbabwe, and South Africa. By Pierre du Toit. Washington, DC: United States Institute of Peace Press, 1995. 355p. $37.50 cloth, $17.95 paper.

James R. Scarritt, *University of Colorado at Boulder*

Utilizing a modified version of the state-society framework presented in Joel Migdal's (1988) *Strong Societies and Weak States*, Pierre du Toit analyzes the prospects for sustainable democracy in the divided societies of Botswana, Zimbabwe, and South Africa. His major modifications of Migdal's framework are distinguishing between state and regime (the latter being the rules that govern the ordering of power within and among state organizations), adding the concept of ethnically divided societies, and focusing explicitly on democratization. He argues that strong states in divided societies are either autonomous or ethnic, and that construction of the former must precede or be concurrent with democratization of regimes and the construction of strong and truly civil societies. Strong autonomous states can create economic growth by manipulating the country's position in the international economy, however unfavorable that may appear to be.

Utilizing this conceptual framework, du Toit correctly argues that none of these three states fits into Robert Kaplan's dismal portrait (Robert D. Kaplan, 1994, "The Anarchy," *The Atlantic Monthly*, February 1994, p. 44) of the universal decay of African states, but that they vary considerably in state strength and autonomy and societal strength and civility, and thus in their potential for attaining the closely related goals of sustainable democracy and economic growth. Botswana is strongest in these regards and Zimbabwe is the weakest, with South Africa—where the pattern of change is most uncertain—falling somewhere in between. While this ranking and the complex and finely nuanced arguments on which it is based are generally well founded, there are several points in du Toit's analysis about which questions can be raised. Unfortunately, this has to be done by presenting only a small fraction of the points that he makes.

Botswana, which is described as a potentially rather than an actually divided society, has constructed a strong state and a strong society over a period of many decades, starting from a very weak base during the colonial period. It has more recently created a stable if imperfect democratic regime. Economic and educational backwardness in the years shortly before and after independence actually facilitated these processes by allowing politically neutral expatriate officials to strengthen the state in alliance with a biracial economic elite of large cattle farmers, beginning with the creation of the Botswana Meat Commission, and by allowing this strong state to promote substantial economic growth by maximizing the returns on its wholly dependent position in the world economy. The significance of income from diamond mining is acknowledged but not fully incorporated into du Toit's analysis; the same is true of the limits on effective democratic participation by the poor in a regime that largely isolates the state from popular control. He sees the major threats to democratic sustainability as environmental degradation and patronage, rather than inequality or revolt from below.

Since most of the book was completed in late 1993, the 1994 elections in South Africa are covered only in an epilogue, events since those elections are not covered at all, and the analysis of the prospects for sustainable democracy in that country is slightly dated. Apartheid strengthened the South African state in some respects while weakening it in others; the state lost autonomy in the process, becoming an almost purely ethnic state. Society was further divided and weakened at the same time, a process that was continued by the struggle against apartheid, which also weakened the state. Du Toit thinks that it is not clear to what extent these changes have been reversed by the negotiations leading to democratization and the institutions emerging from them. The keys to democratic sustainability in South Africa, therefore, are strengthening the state in its relations with the black population, preventing future revolts modeled on the one against apartheid, providing abundant public goods in an equitable manner while maintaining state solvency through economic growth, and making South African society truly civil rather than divided.

Du Toit characterizes the Rhodesian state as an ethnic state that was weaker than its South African counterpart because of the relatively and absolutely smaller size of the dominant group. After independence the Zimbabwean state grew stronger but not more autonomous, confronted rather than worked with its dependency, and adopted an adversarial state ethos not conducive to civility in the society. It was thus unable to produce the levels of either democracy or economic growth that are found in Botswana and may be found in South Africa after a comparable period of independence. While the last part of this argument is essentially correct, some of its premises can be questioned, especially Herbst's argument in *State Politics in Zimbabwe* (1990) that the next generation of whites will have no place in Zimbabwe (an uncivil outcome) and du Toit's implicit assumption that the initial overresponse to the disturbances in Matabeleland in the mid-1980s is prototypical of state policy. More effective policies to promote economic growth and the creation of greater political tolerance are seen as the keys to extending democracy in Zimbabwe.

Underlying this predominantly structuralist argument about the importance of strong and autonomous states and strong societies for sustainable democracy is a rationalist argument about the need for incentives to achieve civility (moderation) and for incentives of politically dominant groups and political parties to provide the former incentives for themselves as well as their opponents. The latter, according to du Toit, must be based in mutual perceptions of scarcity and opportunity to reduce that scarcity by manipulating a dependent position in the world economy. This combination of sophisticated macrotheory and solid microfoundations means that this book will make a significant contribution to thinking about democratic sustainability in Africa, even for those who do not agree with every point made in it.

Anatomy of a Dictatorship: Inside the GDR, 1949–1989. By Mary Fulbrook. New York: Oxford University Press, 1995. 307p. $29.95.

Helga A. Welsh, *Wake Forest University*

The scholarly investigation of former dictatorships constitutes one of many attempts to understand the past and to avoid its repetition. This approach is particularly evident in the unified Germany where special research centers were created to analyze the history of the GDR. In addition, new archival rules are in force that allow access to primary sources without the usual time constraint of a 30-year waiting period. However, given the highly political context in which much of the writing takes place, it is not surprising that the debate is at times quite controversial. At the same time, many newer, empirically based studies aim at avoiding black-and-white pictures and highlight the complexities of modern dictatorships. Mary Fulbrook, Professor of German History at University College London, fully supports this differentiated approach. Her study is a welcome addition to the growing body of literature on the former GDR, especially since it is among the first books written in English that make use of new archival sources.

The author's main goal is to shed light on the nature of dictatorship in the GDR, but her analysis will be of interest to all those who explore the question of how seemingly stable and quiescent states can survive over long periods of time and then surprise with their sudden collapse. In the course of this analysis, Fulbrook aptly challenges interpretations that emphasize exclusively either the evil or the benign character of rule in the GDR. Keeping with her interest in explaining dictatorships, her book is organized around the themes of domination, complicity, and dissent.

Repeatedly Fulbrook refers to the newly available archival material as a "gold mine" that is there to be explored. Not surprisingly, references to the nature and content of some of those sources constitute one of the highlights of her analysis. The author avoids chronological accounts but conveys an illuminating picture of the changes that occurred in the 40 years of East Germany's existence. In many ways she sees the 1970s as a turning point in the political development of the country. Closely connected to the leadership change from Walter Ulbricht to Erich Honecker in 1971, public expectations were first raised and then frustrated. The brief "golden age" (p. 172) of the GDR in the beginning of the 1970s gave way to increased pressure for change. Many of those challenges, as Fulbrook points out, were not isolated but linked to developments in neighboring countries as well. The social contract that had become a hallmark of all communist-governed systems faltered under economic stress and increased societal discontent.

Much of this information is not new, but some of the facets clearly are. For example, the author points out that the reporting of regional and local party and state organs to the center in Berlin varied considerably over time. In particular during the 1950s points of contention were still expressed, whereas in the 1970s the "rhetoric of reporting becomes ritualized" (p. 73). This finding, which is valid for other areas as well, is in itself significant for an interpretation of the inner workings of the regime and its growing inability to adjust to changing conditions in the political and economic environment.

The archival sources also reveal a much higher incidence of work stoppages and "splutterings of revolt" than had been commonly assumed. Yet in the aftermath of the failed June 1953 uprising they remained localized (p. 155). Thus, dissent was fragmented until the late 1970s when "the culture, the character, and organizational forms of dissent changed" (p. 276). As was true for much of Central and Eastern Europe and the former Soviet Union, limited reforms in response to the political and economic challenges finally led to the demise of communism in its "real existing" variant. Thus, the phenomenon of reform and collapse was not limited to the GDR but was part of developments in the communist bloc. However, in the case of the GDR the roles of the West German counterpart as a contributor to its instability and of the Soviet Union as long-standing guarantor of its existence cannot be ignored. Mary Fulbrook fully supports those in the (predominantly German) debate who assert that increased contacts between East and West Germany in the 1970s and 1980s proved detrimental and not beneficial (as some claim) to the stability of the GDR.

The author concludes that "conformity counterbalanced by the retention of a certain inner distance" characterized most people's attitudes toward the communist regime in the GDR (p. 275). She dismisses notions that dictatorships are instituted predominantly on force or even the use of force, but concedes that much of the stability of the GDR was based on a particularly efficient use of domination by a regime that was characterized by its persistent and high degree of insecurity toward the ruled.

Fulbrook is equally forthcoming in rejecting cultural explanations that citizens of the GDR—or for that matter Germans more generally—are particularly prone toward obedience. In addition, she reminds us that "the presence of popular discontent alone can explain neither a revolution nor a lack of revolution or unrest" (p. 270). Her analytical skills are strongest when she explains the seeming paradox of outward stability and conformity *and* insufficient legitimacy, immunity, and limited dissent. Her account is noteworthy as much for its balanced judgment as for its secure grounding in primary and secondary sources. In sum, *Anatomy of a Dictatorship* is a highly valuable book that contributes to a better understanding of the history of the GDR and that of other dictatorships.

Engendering the Chinese Revolution: Radical Women, Communist Politics, and Mass Movements in the 1920s. By Christina Kelley Gilmartin. Berkeley: University of California Press, 1995. 305p. $40.00 cloth, $15.00 paper.

Jean C. Robinson, *Indiana University*

The Chinese Communists periodically promote new policies aimed at gender reform. The old assertion that "women hold up half the sky" and the new assertion that sexual equality is rapidly being achieved rest on the claim of the Chinese Communist Party (CCP) that it has consistently promoted the revolutionary restructuring of sex and gender in China. In the past twenty years that claim has been critiqued in a number of important studies of women and gender in the revolutionary as well as contemporary policies and politics of China. Political scientists, historians, and sociologists have found discrepancies between China's lauded policies and practices at the local level. Discordant trends that both promote and inhibit gender reform have appeared and reappeared over the decades. Until *Engendering the Chinese Revolution*, many researchers have assumed that there were fundamental contradictions between CCP policy, strategy, and practice. Now, with the publication of Gilmartin's study, we have more solid evidence of the ways in which the early years of the CCP set a pattern that we continue to see in contemporary China.

The Chinese national and communist revolutions have been extensively studied. Yet despite this wealth of significant research on the revolutionary strategies and practices of the CCP, no Western scholar has put women's political participation in the very early years of the revolution at center stage. Gilmartin does this, and in so doing she provides answers to some of the vexing questions uncovered by previous research.

Researchers have regularly noted the tension between the protofeminist values of the Chinese revolution and the rather inconsistent policies of Chinese Communist leaders. What explains this anomaly? Not coincidentally, the remarkably few women who have made it to the upper echelons of the party are frequently contrasted with the mobilization efforts to promote women's mass participation in the revolution. Why haven't more women made it to the top? Is the CCP claim about gender equality mere rhetoric? How can we understand these phenomena?

Gilmartin paints a carefully detailed history of the radical women involved in the revolution and communist politics from 1920 to 1927. In addition to providing an excellent biographical addendum and lively narratives of the contributions of the major female activists, such as Xiang Jingyu, Yang Zhihua, and He Xiangning, Gilmartin uncovers numerous ways in which the political opportunity structures of the 1920s cast gender politics in certain directions. As an historian, Gilmartin does not use the language of political opportunity to conceptualize what she is describing, but she is conscious of the ways in which the ideology and opportunity structures of the May Fourth Movement (1919), the May Thirtieth Movement (1925), and the united front between the Nationalists and the Communists enabled and constrained political participation. Gilmartin notes, for instance, the greater ease, compared to later radicals, that May Fourth feminists (both male and female) brought to discussions of sexuality, marriage, and divorce. She is correct, I believe, in suggesting that the radical discourse of that time, and its urban character, allowed men and women to promote sexual liberation in ways that were not acceptable during the united front in the mid-1920s or in rural organizing. In the latter cases, the social conservatism of some Nationalists and the patriarchal and feudal nature of rural society made revolutionary mobilization much more difficult once sexual liberation issues were raised. So these issues were downplayed, or even ignored, so that the "larger" revolutionary cause could be promoted. Thus, the ways in which women were mobilized and the kinds of gender issues that were publicly discussed were a product of the radicals' assessment of the barriers to their political success. Encouraging women to participate in politics, debating female sexuality and divorce, and even the promotion of marriage reform were subject to political (and not necessarily feminist) decisions about what would provoke hostility in a particular setting. The women's agenda was always subordinate to the broader political interests of the Communists.

Gilmartin also extensively demonstrates the ways in which women activists' careers were constrained by the assessment of a woman's value in terms of her male partner's political currency. Again and again the status of one's husband or lover within the CCP hierarchy has been a determinant of a woman's appointment to major positions within the Central Committee, senior advisory groups, and indeed even the women's bureaux. Every woman who has served in a leading role in the CCP women's organization has been married to or closely affiliated with a major male leader. Once a husband is demoted or a woman divorces a prominent spouse, the female communist activist loses her place within the hierarchy. Gilmartin points to the ways in which women's political participation in the revolutionary apparatus is dependent upon male status and suggests, quite rightly, that this is at odds with the promotion of women as independent political actors. Women's agency is thus proscribed by male preference.

Finally Gilmartin elucidates vividly the ways in which women's relatively small role in revolutionary activism paradoxically became a symbol for the structural changes attached to gender reform. After the CCP–Nationalist breakup in 1927, women radicals were specifically attacked and tortured by the Nationalists not only because they were part of the "Red" opposition but also because they were understood to be attacking the foundations of patriarchal culture and tradition. Speaking out publicly, seeking an identity separate from their fathers and husbands, wearing uniforms and toting guns, promoting sexual choice and marital freedom were all undeniable assaults on the male-dominated social order. By raping women activists, amputating women's breasts, and sexually mutilating women, the "White terror served to repress the women's mass movement and to vividly demonstrate the penalty for political participation" (p. 212).

Engendering the Chinese Revolution enables us to see how context shapes the ways in which political mobilization and participation can happen or not. Here, too, we see how the political involvement of women, even if they are not feminist, can be interpreted as a war waged against the very foundations of society. Gilmartin understands how critical women's political roles are to modernization, to revolution, and to those who want to preserve the status quo. This history of women's involvement in early communist politics provides an excellent analysis of radical women's contributions, the progressive impulse of male CCP leaders, and, ultimately, the structural impediments to women's exercise of agency and full political participation in China.

Rights of Passage: Struggles for Lesbian and Gay Legal Equality. By Didi Herman. Toronto: University of Toronto Press, 1994. 198p. $60.00 cloth, $19.95 paper.

Shane Phelan, *University of New Mexico*

Lesbian and gay politics is a new and fertile field for research on social movements. *Rights of Passage* is a fine example of the new scholarship. Didi Herman addresses the relation between social movements and the law, specifically the ways in which movements translate, mask, or transform their agendas for legal consumption and the effects of those moves on the movements themselves. To explore this problem, Herman focuses on two battles in Canada over gay and lesbian rights: the effort in the mid-1980s to include sexual orientation in Ontario's Human Rights Code (Bill 7) and the *Mossop* court case concerning benefits based on "family status." Through interviews with gay and lesbian activists and with the leaders of the three New Christian Right organizations that mobilized in the *Mossop* case, examination of the court documents, and readings of public media and internal organizational/community writing, Herman documents the slips between "insider" understandings, media presentations, and legal framings of public issues.

The overriding issue of the book is the problem of "rights discourse" as a vehicle for social transformation. Herman frames the Bill 7 debate as an example of the hegemony of the "minority rights paradigm" (p. 5), in which lesbians and gays are portrayed as a fixed, discrete minority group. For many this has been an attractive alternative to charges of sin or sickness, but Herman cogently argues that the price of the minority paradigm is a narrowing of social vision and the reinstitution of disciplinary regimes of sexuality. This discipline is evident in the legal reliance on experts from psychology and sociology who are called to testify on matters of homosexuality. Psychology-based experts testify as to the features of homosexuals and homosexuality, never challenging the normative status of heterosexuality; sociologists, historians, and (increasingly) political scientists find themselves boxed within minority discourses in the courtroom, forced to translate the subtleties of social construction into yes/no statements about family structure, social stability, and sexual variation. (Herman's book was written before the testimony concerning Colorado's Amendment 2, in which political scientists were called to establish the political power and powerlessness of gays and lesbians; I suspect her reservations would hold against those cases as well.)

The book is noteworthy as much for its coverage of the Canadian New Christian Right (NCR) as for its treatment of lesbian and gay activism. Two chapters on the NCR focus on the worldview of the NCR and on the ways that that worldview was muted for the purpose of legal intervention in *Mossop*. Herman astutely describes the ways in which both the NCR and gay and lesbian leaders are frustrated by the limitations of rights discourse. Indeed, the NCR leaders are more attuned to a rights critique than the gay/lesbian activists, because the latter see rights as at least protective, while the NCR is frustrated in its aims. The NCR leaders are involved in a communitarian discourse of responsibilities and privileges, and see rights as fragmenting and oppositional. Lesbian and gay activists often were aware that rights discourse works through fixing differences and identities, but chose to use it "because it was there" (p. 58), and their concern for immediate gains against discrimination outweighed longer-term or more theoretical concerns. Herman argues that these "theoretical" concerns are in fact central to how lesbian and gay movements present their visions. We cannot adopt medical, biological, or psychological models in the courts without having them spill into our daily lives, often in forms we would not endorse.

Herman's interviews have provided much worthwhile material, and she clearly presents the complexities of their ideas and the conflicts among the groups. In contrast, her treatment of activists in the Coalition for Gay Rights in Ontario (CGRO), the primary actors in the Bill 7 fight, is more cursory; Herman gives relatively short shrift to the interviews and discussion that might have really dramatized the gap, so evident in the NCR legal brief, between "internal" community views and public presentations. This is not a major problem for those versed in lesbian and gay politics, but it may be deceptive for those new to the field.

Another limitation in Herman's presentation concerns her isolation of struggles over lesbian and gay rights from other political events in Canada at the time. In assessing the relative gains of the NCR and CGRO over Bill 7, she argues that CGRO's gains were due to the growth of support for equality within the Liberal and New Democratic Parties. Thus, when the Liberals came to power, Bill 7 was greeted more warmly than would otherwise have been the case. This is certainly important, but Herman labels "such events" as "unpredictable 'chaos' phenomena" (p. 71). Her point seems to be that CGRO cannot take credit for the Liberal/NDP accession, and that much is certainly true; nonetheless, such events are neither unpredictable nor unrelated to Bill 7's success. Herman notes that without such changes Bill 7 "might never have passed" (p. 70), but she does not sufficiently engage the meaning of the accession and the growing acceptance of gay/lesbian equality within the two parties. Rather than write off these changes as "unpredictable," readers may hope for a more thorough discussion of the activism before 1985 that fostered such changes and an integration of the Bill 7 victory within that context. This is not a major objection to a book rich in details about legal strategies, but it is a problem common to all scholars of social movements. We can hope the next generation of comparativists, trained in less hostile atmospheres, will offer us such integrated analyses.

Fortunately, these problems are minor compared to the wealth of information and analysis that Herman offers. For scholars in law and social movements, Canadian politics, and lesbian and gay politics, *Rights of Passage* will be an invaluable volume.

The Japanese Civil Service and Economic Development: Catalysts of Change. Edited by Hyung-Ki Kim, Michio Muramatsu, and T. J. Pempel, New York: Oxford University Press, 1995. 552p. $65.00.

Robert C. Angel, *University of South Carolina*

The World Bank's Economic Development Institute, with funding from the government of Japan's Human Resources Development Trust Fund, joined forces with the University of Washington-based Society for Japanese Studies to undertake this ambitious project. The second in a series of studies on the Asian economic development experience, it follows the institute's widely debated *East Asian Miracles* (1993). The editors have recruited eighteen highly qualified scholars to write on those topics and from those intellectual perspectives they considered appropriate for inclusion.

The editors note they write for the managers of economic growth in developing and transitional economies around the globe, rather than for academic specialists on Japan or comparative political economy. They present their interpretation of the role of the bureaucracy in Japan's modern economic development to this audience in four sections. Three longish articles in the first section by political scientists T. J. Pempel and Michio Muramatsu, law professor John Haley, and economist Kozo Yamamura set the stage. Pempel and Muramatsu explain how interpersonal competition within an overall cooperative bureaucratic framework has been harnessed to forge a bureaucracy that has managed to remain small, efficient, and creative, a far cry from the usual image of governmental bureaucracy. Haley explains how an administrative legal framework that grants very weak coercive powers to the civil service has compelled bureaucrats in pursuit of voluntary compliance with their directives to develop better relations with the private sector, relationships that also have facilitated rapid economic development. Yamamura then traces the role of the Japanese bureaucracy in national economic development since the early years of the mid-nineteenth century onward, noting its intervention in the market where beneficial, especially to reduce information acquisition costs and to stimulate efforts to catch up with the West. Yamamura identifies as most important the bureaucracy's support of technology acquisition from the West, reduction in the cost of capital for firms and industrial sectors judged to have greatest potential, promotion of exports, and obstruction of imports.

The remainder of the book includes a five-article section that illustrates critical aspects of the evolution of the bureaucracy's relationship with the private sector, a series of five cases that illustrate how the bureaucracy dealt with specific challenges, and a concluding comparative section in which the Japanese experience is considered in light of the experience of other countries and regions.

Space limitations preclude individual comment on each of the articles. But Inoki's interpretation of the effect of early retirement of senior bureaucrats and their re-employment in the private sector (*amakudari*) as a source of competitive motivation within the civil service and Ito's analysis of the consequences of political reform are particularly useful. Here too, former Economic Planning Agency official Yutaka Kosai, in his piece on government-business relations, attacks the "Japan Inc." model, propping up and breathing new life into that discredited and desiccated corpse, assuring its survival into the next century.

The case studies include useful explanations of how Japan's civil service has handled problems common to all government managers in the course of economic development, such as financing, central-local government relations, and labor policy. Much of this information also will interest specialists on econo-political Japan. Meredith Woo-Cumings's essay in the comparative section, one of the most interesting in the volume, should prove quite helpful for the project's economic developer target audience, since she explains how a developing economy has taken lessons from Japan throughout much of this century, whether it wished to or not. Accepting without preamble the importance of the government's market intervention for economic development in Korea, Woo-Cumings explains in a straightforward fashion the effect of Japanese influence on Korea and how this influence combined with indigenous traditions.

Mindful of cautions of Japan's unusual attributes and environment that run throughout their articles, the editors in their introduction and Hyung-Ki Kim in the final chapter still conclude that managers of developing and transitional economies can profitably study the economic development role of Japan's civil service. Especially important is the effect of encouraging competition within the bureaucracy at the individual, intraministerial, interministerial, and central-local governmental levels, producing, in contrast to traditional models of lethargic bureaucracy, more efficient, nimble, and creative institutions. The editors also emphasize Japan's respect for the market. Yamamura notes that in spite of the reputation of Japan's civil service for protectionism and an anti-market orientation, it has "built in certain features of market competition that have been highly beneficial to the national economy and to long-term national economic growth" (p. 15).

No topic in the study of econo-political Japan inspires more controversy than the role of the bureaucracy in the national economy. Neoclassical economists often find it difficult to consider government as any more than a temporary interference in market allocations of values and one of little long-term consequence. Political scientists who emphasize the pluralistic tendencies of Japanese politics are inclined to look with suspicion on interpretations that discount the influence of representative governmental institutions over powerful ministries and agencies. After a flush of pride in the early 1980s, Japan's bureaucrats fear being portrayed, especially in English, as too influential, lest such characterization inspire American trade negotiators to demand they shift their influence from promotion of the chronic bilateral trade imbalance to its correction.

Thus, analysts of Japan's political economy who emphasize the significance of its civil service in the national economy have come to be known among their critics as "revisionists and Japan-bashers," to borrow an illustrative phrase from the book (p. 190). Refutation of revisionist interpretations of Japan's political economy occupies considerable space in many of the articles in the volume. The anti-revisionist enterprise certainly belongs in a study of this sort. But, considering the collective effort expended, it would have been simpler to include one or two articles that accurately represent revisionist ideas rather than to rely on interpretations that border in places on caricature. Indeed, given their emphasis on the economic role of the bureaucracy, a modest revisionist presence would have made the volume more complete and might even have increased its value for the intended audience.

One avoids suggesting additions to densely packed volumes of over five hundred pages. But two come to mind here that would further elucidate the role of the civil service in Japan's modern economic development. The first would be an article devoted specifically to administrative guidance, a topic mentioned in several of the essays. But given its significance in bureaucracy–private sector relations and the diversity of interpretation it has inspired in the literature, an article devoted specifically to administrative guidance would have been welcome. My second candidate for an article topic would be democratization. It too is mentioned by Pempel and Muramatsu, Silberman, Mabuchi, and others. But an article devoted specifically to democratization that explored the consequences for representative political institutions and democratic governmental processes of efforts to strengthen the bureaucracy would have been welcome. It is noteworthy that the only mention of the role of police forces in the whole volume appears in Woo-Cumings's discussion of Korea.

While this volume neither is nor presumes to be the final word on the role of Japan's civil service in economic development, its harvest of information and assessment represents a most welcome addition to the English language literature on Japan's political economy. It seems to me more useful for the English-speaking specialist on Japan than for the latter day economic developer. But future citations will tell.

The Reincarnation of Russia: Struggling with the Legacy of Communism, 1990–1994. By John Loewenhardt. Durham, NC: Duke University Press, 1995. 238p. $42.50 cloth, $15.95 paper.

Nicolai N. Petro, *University of Rhode Island*

In this short book, John Loewenhardt, a distinguished Dutch observer of Russian and Soviet politics, sets out to test the alleged uniqueness of Russia's transition to democracy. His aim, as he points out, is not to explain the collapse of the Soviet Union, but to examine the problems of Russia's emergence from the collapse of the USSR.

Loewenhardt's first chapter is by far the best. In it he summarizes the key approaches to democratic transition that have been applied to authoritarian regimes in Latin America and Eastern Europe, and asks whether some of these models might not also apply to Russia. Despite the confusion and volatility of contemporary Russian society, Loewenhardt argues that the problems Russia faces in her transition to democracy are not sui generis. Russia's approach to democracy may be distinctive, but not sufficiently so that models applied elsewhere cannot also be applied fruitfully.

Playing devil's advocate, Loewenhardt then launches into an exploration of the key issues that condition Russia's experience of transition. These include the enduring legacy of Soviet attitudes and institutions, the constraints of Russian history and political culture, and the impact of generational change on Soviet society. Subsequent chapters highlight the problems that the collapse of empire pose for

Russia's transition to democracy and recount the crisis of institutions and legitimacy. While these chapters do not offer much new information, they make the case that while the transition to democracy in Russia has been characterized by factors that are distinctively Russian, these factors are not uniquely Russian.

Without minimizing the many problems that remain on Russia's road to democracy—an excessively large and bureaucratic state apparatus, a middle class uncertain of its prospects, a demoralized population, the extensive criminalization of politics—Loewenhardt also points to the significant strides that have been made toward the consolidation of a new political consensus among the elite, the gathering stabilization of the Russian economy, and the strengthening of civil society. His conclusions are therefore cautiously optimistic: despite the challenges that lie ahead, Loewenhardt expects Russia to lurch forward in her search for democratic stability.

The book's greatest merit lies in the author's analysis of the literature on comparative democratic transition and his efforts to apply this literature to Russia. While he does a fine job discussing key aspects of that transition in other countries, he does not do a very good job of applying these lessons to Russia. His conclusion, namely, that Russia may now indeed be considered a democracy (albeit a "minimal democracy"), would have been strengthened if, at the end of his book, he had returned to his original discussion of the literature on transitions and given us an indication of which model (or combination of models) he feels is the most appropriate for understanding democratic transition in Russia. As it stands, the arguments for treating Russia like other transition regimes seem supported, ultimately, only by the author's own inveterate optimism.

Despite its shortcomings, this book raises a number of important questions for students of comparative politics, not the least of which is whether Russia has already made the basic societal transformations that one expects of democratic polities.

The Sandinista Legacy: Lessons from a Political Economy in Transition. By Ilja A. Luciak. Gainesville: University Press of Florida, 1995. 258p. $49.95.

Theory in the Practice of the Nicaraguan Revolution. By Bruce E. Wright. Athens: Ohio University Press, 1995. 272p. $23.00 paper.

Gary Prevost, *St. John's University*

What is the meaning of the Sandinista revolutionary experience for Nicaragua and for students of Latin American politics and revolutionary change? These works by Luciak and Wright add significantly to our understanding of a revolution that has received considerable attention from both scholars and political leaders over the twenty years since the Sandinista National Liberation Front came to power in a popular uprising in 1979 and then ceded power peacefully following a surprising electoral defeat in 1990.

A theme that unites both works is the belief that the Nicaraguan revolutionary process, while significantly damaged by the electoral defeat and the subsequent counterreform of the administration of Violeta Chamorro, has not been eradicated from Nicaraguan history. Referring to the revolutionary process, Wright says "initial reports of its death were premature, failing to take into account the extent to which the Nicaraguan population has come to be a force of its own" (p. 27). Both writers cite the 1990 elections as a victory for the democratic process. Luciak states: "Many observers have argued that the Nicaraguan revolution failed. I would maintain the opposite. The fact that the revolutionary regime was prepared to accept the verdict of the people in an electoral process legitimizes the Sandinista project" (p. 183). Wright adds that the democracy establishes a safe political space in which the FSLN can operate in an attempt to regain political power in the future.

Both authors praise the Sandinista commitment to electoral democracy, but they acknowledge that such change was not the primary goal of the young revolutionaries who launched a guerrilla struggle in the early 1960s. Luciak poses the most important question: "The Sandinistas came to power with the promise to establish social and economic democracy.... Did the revolution fulfill its promise to the Nicaraguan people?" (p. 184). Luciak and Wright do not offer a definitive answer to this question, arguing plausibly that the changes set in motion during the Sandinista years in power have yet to run their course. However, both authors are not afraid to criticize the Sandinistas for their policy failures and to acknowledge the role that the U.S.-sponsored contra war played in limiting Nicaragua's horizons.

The central theme of the Luciak volume is an attempt to understand how the Nicaraguan revolutionary leaders, who were committed to the long-term construction of socialism, attempted a transitional strategy that left a majority of the Nicaraguan economy in private hands. To understand their strategy, Luciak evaluates "the extent to which domestic structures were modified to allow for political participation by the rural proletariat and agricultural producers" (p. 4). As Luciak describes, the Sandinistas conceded considerable economic power to private capital when they assumed power in 1979 at the head of a multiclass political alliance. The FSLN followed the Gramscian prescription of "seeking hegemony" through control of political power. According to Luciak, "the revolutionary government sought to impose 'a logic of the majority' against an entrenched elite" (p. 7).

The primary focal point of Luciak's research was in the Nicaraguan countryside where the Sandinistas sought to transform the political economy using the strength of newly formed grassroots organizations, especially the National Union of Farmers and Ranchers (UNAG) and the Association of Rural Workers (ATC). In addition to the inherent conflict between rural agricultural workers preoccupied with wage and working conditions and propertied peasants focused on prices received at the market, Luciak documents that the Sandinistas, in spite of their stated commitment to grassroots democracy, did not usually foster democratic representation in the mass organizations. Viewing themselves as the "vanguard" of the revolution, Sandinista leaders often closely controlled these movements from the top down. Such top-down control insulated the national party leadership from the real feelings at the base and contributed to the FSLN's downfall in the 1990 elections.

In spite of these limitations, both the ATC and the UNAG made significant contributions during the years of Sandinista state power and afterward. UNAG succeeded in integrating sectors of the agrarian bourgeoisie into the revolutionary project. The ATC had its greatest success in the early days of the revolution before the contra war began to significantly undermine the economy. However, the organization regrouped in the late 1980s, and the author shows how it has become an important organization in the current resistance to the Chamorro counterreform.

Luciak gives a detailed account of one particular project of social and economic transformation—the peasant stores. This project emerged in the mid-1980s as the Sandinistas were rapidly losing credibility and the counterrevolutionary forces were solidifying support in several regions of Nicaragua. A key element of a new strategy to win back the support of rural Nicaraguans was the establishment of a rural supply network with emergency aid from international donors. The rural supply network was targeted to the poorest peasant families in the war zones. Connected directly to UNAG, these stores also provided the basis for peasant organizing and grassroots democracy. The peasant stores often acted in direct competition to government-run operations that had monopolized the market in the years immediately after the revolution. However, the relative success of the peasant stores by 1990 made them an excellent vehicle for independent grassroots action against the Chamorro government's counterreform.

The Luciak volume also contains an excellent chapter on rural women during the Sandinista period and under Chamorro. Rural women constituted one of the most marginalized sectors of Nicaraguan society, and their circumstances have generally not been well covered by previous scholarship. The Sandinista leadership had an ambivalent position on women's issues. For most of the 1980s the FSLN maintained that the defense of the revolution required the subordination of the fight for women's emancipation. As a result, "this position was a key factor in restricting progress toward the full realization of women's rights" (pp. 161–2). However, some legal changes did occur and provided benefit to rural women. A 1981 agrarian reform law gave women the right to own land and to participate in cooperatives under the same conditions as men. According to Luciak, female agricultural laborers did benefit from the Sandinista government's commitment to improve basic needs, especially in education and health care. However, Luciak describes how male prejudice has often limited women to lower-paying jobs (p. 170). He concludes that the difficulties encountered by rural women were illustrative of the general status of women's emancipation in Nicaragua, though there were significant formal legal gains that did not frequently translate into changed practices at the grassroot level, at least in part due to the ambivalence of the FSLN itself. He notes that ironically the women's movement may well be stronger in Nicaragua today, freed from the limitations of ambivalent Sandinista state policy.

While Luciak's work falls squarely within the genre of political economy, Wright tackles important questions of political philosophy and ideology. Wright makes an important contribution in chapter 2 when he analyzes the role of FSLN founder Carlos Fonseca's attempt to establish a theoretical foundation for the Nicaraguan revolution. Any such effort must start with Fonseca's study of Augusto Cesár Sandino, the peasant leader who fought the U.S. Marines in the 1920s. Previous attempts (e.g., by Nolan 1984, Hodges 1986, and Palmer 1988) have made the mistake of seeing Fonseca's study of Sandino as solely an attempt to use Sandino's ideas and actions as inspiration to oppose the Somoza dynasty. Wright correctly sees that Fonseca was also learning directly from both Sandino's ideas and his military tactics. Through the study of Sandino, Fonseca arrived at the view that the achievement of socialist revolution in Nicaragua would occur on its own terms and not on the basis of a model imported from outside. Such a perspective was crucial to Fonseca's break from the Moscow-oriented Nicaraguan Socialist Party (PSN) and his decision to found the FSLN in 1961. In Wright's view, Sandino's praxis also led Fonseca to three basic principles of the FSLN: armed struggle, political organizing among both workers and farmers, and the necessity of a general strike and insurrection to bring down the dictatorship (p. 74).

Wright then proceeds to link Fonseca's ideas to the principles of governance established by the Sandinistas when they gained state power in 1979 three years after Fonseca's death: political pluralism, mixed economy, popular participation, and international nonalignment (p. 78). Unfortunately, this connection is not fully successful. These governing principles did not come from Fonseca or the 1969 Historic Program of the FSLN; rather, they came from the political platform of the Tercerista faction of the FSLN that only fully emerged after Fonseca's death. In my view, the Tercerista faction made important revisions in Sandinista political philosophy that helped bring the revolutionaries to power, but also may have doomed their efforts to remake Nicaraguan society. While the Tercerista adoption of the Western notions of mixed economy and political pluralism allowed the FSLN to court social democratic support, it also helped to create deep contradictions and divisions within the revolutionary process that contributed to the 1990 electoral defeat. Wright has a thoughtful analysis (chap. 3) of the development of the concept of pluralism in liberal thought; however, he ultimately fails to reconcile Fonseca's revolutionary notion of coalition-building to achieve state power with pluralism's inevitable connection to capitalist democracy.

Further research on Fonseca's ideas is now under way using recently opened archives, and we can hope for future works that will enhance our understanding of this important revolutionary figure. If the prediction by both authors that the Nicaraguan revolutionary process will survive proves accurate, we can look forward to many future studies of Nicaraguan society. Both volumes, especially that of Luciak, set an important standard by engaging in thoughtful field research that studies the everyday lives and activities of the people that the Nicaraguan revolution sought to benefit.

Political Reformism in Mexico: An Overview of Contemporary Mexican Politics. By Stephen D. Morris. Boulder, CO: Lynne Rienner, 1995. 261p. $49.95 cloth, $19.95 paper.

Roderic Ai Camp, *Tulane University*

Morris, who previously published the first book on Mexico that wrestled with the complex issue of political corruption, explores in this work the hot topic of political liberalism. Although numerous collections have appeared in the past few years on Mexican political or economic reforms, or on the connections between the two, few lengthy interpretations are available by an individual author. This author's focus is on the specific linkage between political reform and change, and the interaction and consequences of one on the other.

Professor Morris approaches the topic by dividing his discussion into three parts. The first of these, the bulk of the book, lays out the larger political context in Mexico, developing the background to the crisis in that country in the 1980s and 1990s. The second section, and the most innovative, is an effort to explore Mexico's patterns from three comparative angles: subnational, historical, and cross-national. Finally, part three closes with a brief conclusion and epilogue discussing President Ernesto Zedillo's first 100 days in office.

The hypothetical task the author sets out for himself is to determine whether political reforms produce change or instead moderate the forces driving those developments.

The book develops arguments about Mexico's past ability to cope with political crisis—specifically as a long-standing unique case—its current success in remaining largely intact against numerous challenges, and its likely future. The author attempts a conceptualization of reformism, which is clear but too brief. One of the most original contributions in the book is his classification of political actors' strategic choices vis-à-vis reformism. Morris divides such actors into system and nonsystem reformers and opponents, concluding not surprisingly that in the Mexican case, nonsystem reformers exercised the greatest pressure in support of political change. Nevertheless, he also reveals more clearly the interactions among groups within the political leadership and how their relationships are tied to the behavior of regime opponents.

While the author's general appraisal of reforms and the patterns that evolved under two successive presidents (Miguel de la Madrid, 1982–88; Carlos Salinas de Gortari, 1988–94) are essential to understanding what took place in the Mexican case, the most interesting insights are raised in a chapter on Jalisco, a major western state, and the source of critical opposition from the National Action Party. It is one of the few published efforts to place the local within the national context of reform. Jalisco illustrates how the government party initially succeeded in combating reformist patterns from 1988 to 1992, before falling on hard times. Morris suggests that Salinas bolstered government support among those groups where traditionally it had done poorly, but lost support among other groups where it had typically claimed success.

One wishes that Morris had expanded his interpretations of the consequences of reform and the comparisons between the two presidential administrations. However, his comparative chapter with Eastern Europe and other cases will be of great use to other scholars, especially nonspecialists. Although many similarities exist among reformist experiences in the international context, the author concludes that Mexico's peculiar historical circumstances and its geographic proximity to the United States explain features unique to its own brand of reformism.

Ultimately, Morris views reformism as a disjointed project that combines the old with the new. He argues persuasively that the Mexican case identifies special features that affect this process, most significantly presidentialism, autonomy between political and economic reform, limited pluralism, and a strong but malleable state ideology. Mexico is cast as an adaptive, authoritarian regime. The insights provided in this thoughtful, interpretative work shed much light on recent Mexican political patterns and will help to explain its future direction.

Military Rebellion in Argentina: Between Coups and Consolidation. By Deborah L. Norden. Lincoln: University of Nebraska Press, 1996. 242p. $35.00 cloth, $17.50 paper.

Gerardo L. Munck, *University of Illinois at Urbana-Champaign*

One of the challenges of democratic consolidation stressed in the literature on the third wave of democratization is the successful management of the military under democratic control. This challenge is, to be sure, one among many. Others that have gained attention include the imperative and consequences of structural economic reform, the threat to the territorial integrity of the state posed by identity politics, and the need for adequate political parties and constitutional structures. As Linz and Stepan (1996) argue in *Problems of Democratic Transition and Consolidation,* however, in cases where the ancient régime was led by a hierarchical, rather than a nonhierarchical, military elite, the problem of civil-military relations is likely to be a particularly ominous one. The argument is that where the military institution controlled the reins of power, they will be able to retain prerogatives and establish reserve domains that run counter to the subordination of the military to popularly elected authorities, a defining feature of democracy.

This was not the case, however, in Argentina, the country studied by Norden, even though it completed a transition from bureaucratic-authoritarian (BA) rule, a form of authoritarianism in which the military institution controlled the government. Instead, because of the military's failures in government and an abrupt mode of transition, Argentina's military rulers were not able to impose the kind of confining conditions that their counterparts did in Brazil and even more clearly in Chile. Indeed, democratic rule in Argentina began in 1983 with the military in a relatively weak position vis-á-vis the new civilian rulers. Still, this positive situation did not directly lead to the military's subordination to civilian authorities and, as Norden shows, Argentina's new democracy was shaken by its own peculiar military problem, which took the form of four military rebellions during the 1987–90 period.

Focusing on these military rebellions, the first key point that Norden establishes is that the distinctiveness of civil-military relations in post-1983 Argentina lay in that the government was challenged not by the military hierarchy itself but by a group of middle-ranking officers within the army. In other words, military insubordination to civilian rule did not assume the form of a unified military institution seeking to retain undemocratic powers at the expense of the civilian authorities, as Linz and Stepan stress, but was rooted rather in military factionalism and challenges to a military hierarchy that coöperated with the civilian government.

The core concern of this book, however, is to explain the emergence and evolution of the rebel movement, commonly referred to as the *carapintadas,* that was behind these uprisings. Succinctly, the initial organization of the rebel military movement is explained in terms of cleavages within the military bureaucracy, which reflected differentiated positions within the military hierarchy as well as distinct military functions and which were politicized as a result of the cumulative impact of the military's involvement in government during the 1976–83 period (chaps. 3 and 5) and policy toward the military during the Alfonsín government (1983–89) (chap. 4). More specifically, the factors that politicized latent cleavages included, prior to 1983, the military's involvement in repression and in the conduct of government; with the military's defeat in the Falkland/Malvinas war; and thereafter the actions of the Alfonsín government, the civilian courts and human rights groups. Once the movement had emerged, its evolution and eventual split into a military wing, which was eventually defeated in 1990, and a political wing, which succeeded in forming a political party, responded to the changing composition of, and demands by, the rebel forces (chap. 6) and the policies of the Menem administration (1989–95), which included a rapprochement with the armed forces that reinforced the resolve of the military hierarchy to take a tougher stance toward the rebels and ultimately use military force to put down their rebellion.

This explanatory argument is complex but clearly presented. It is supported, moreover, on the basis of careful research, including interviews with many of the key players and a close reading of military publications. Norden's book stands out, indeed, as the most thorough and insightful analysis available of the role of the military in post-1983 Argentina. In addition, by showing that civil-military relations in post-BA contexts cannot be reduced to the challenges presented by the military hierarchy and by explaining how a threat to democracy originating in military factionalism can arise and evolve, this book deserves to be read for its broader contribution to the study of civil-military relations in new democracies.

The important contribution made by this book notwithstanding, there are two issues linked to the questions Norden addresses that could have been addressed more explicitly. The first concerns the issue of human rights, or more precisely the value of justice, in posttransitional settings. Norden shows how the origin of Argentina's military rebellions lay in part in factors internal to the military, but also in part in the policies pursued by Alfonsín, the courts, and human rights groups. As Norden shows, these policies contributed to military rebellion because they were viewed by the rebels as acts of retribution. By emphasizing how the military interpreted such civilian acts, however, Norden does not give enough attention to the legitimacy, within the framework of democracy, of the civilians' acts. That is, it remains unclear whether the acts that are seen as retribution by the rebels may nonetheless be part of the pursuit of a just cause by civilians: the punishment of military officers who committed egregious human rights violations.

The importance of this issue is that the lessons of civil-military relations are quite different when viewed from a civilian instead of a military perspective. In favorable cases like Argentina, where the military was not able to retain prerogatives and where the balance of power clearly favored the civilian authorities originally, the lesson seems to be that civilian rulers must buy military quiescence through the granting of amnesty for past acts. In more conditioned cases, such as Brazil and Chile, the lesson is even more dismaying, given that the advisable policy on the part of prudent civilian rulers would be to respect prerogatives the military succeeded in imposing during the transition, in effect accepting that the best civilians can do is offer a symbolic response to the human rights question. The lessons, in short, point to the need to stress the dilemmas generated not only by the actions but also by the externally or self-imposed inaction of democrats as they deal with "the past" and to the problem for democracy posed not only by military reactions to civilians but also by the a priori limits the military imposes on civilian action. (While dealing with the past takes the form of confronting the human rights question in post-BA democracies, it assumes different guises in accordance with the nature of the prior nondemocratic regime in other cases. The dilemmas, however, are quite similar.

The second issue concerns the explanatory factors stressed in this book. Norden argues that an explanation of military intervention in politics must stress internal factors, that is, those concerning the armed fores themselves. This is probably correct, and her book certainly shows that nuanced arguments about the military must rely heavily on such internal factors. It is another matter, however, to suggest that the broader context within which the military acts or their alliances with civilians should not be given equal attention (pp. 4, 13–8). Indeed, there are two difficulties that follow quite directly from such a conceptual restriction. First, there is the problem of explaining why military discontent in Argentina and the threat to democracy it presented was in some way bounded and did not lead to an outright military coup d'état. Such an explanation, it is reasonable to venture, would have to consider nonmilitary factors, such as the decimation of the left during the prior period of military rule, the increasing friendliness of government policies to business interests, and the end of the Cold War.

Second, there is the critical problem of assessing the relative impact of civil-military relations within the overall process of democratic consolidation. With regard to this point, Norden acknowledges that the military problem is but one among several factors likely to shape the prospects of democratic consolidation. But lacking a broad conceptual framework that links purely military matters to other social and political concerns, she is unable to offer any systematic assessment of just how much of a threat to the overall process of democratic consolidation developments like the military rebellions that occurred in Argentina actually represent, and whether, say, these developments are more or less of a problem than those associated with the imperative of economic reform. This analytical task, to be fair, goes beyond the purported scope of Norden's book and is mentioned here not by way of criticism but rather to highlight what remains a conceptual challenge for the field of democratization studies as a whole. Indeed, by working through one dimension of a larger problem with great meticulousness and clarity and by providing essential building blocks in the large puzzle called democratization, Norden's book constitutes an invaluable source to those who are grappling with these big and important questions.

State and Society in China's Political Economy. By Chih-Yu Shih. Boulder, CO: Lynne Rienner, 1995. 207p. $45.00.

Yasheng Huang, *University of Michigan, Ann Arbor*

This volume explores the relationships between China's political culture and its economic reform policies and outcomes. The approach is speculative rather than rigorously empirical. The author argues that Chinese political economy in general and economic reforms in particular can be understood better by taking into account Chinese cultural background. He boldly asserts that to understand reform in China "is to feel it." This explicit emphasis on "empathy" as an analytical tool sets it apart from many books on similar topics, and in the preface to the book the author bemoans the fact that this kind of cultural approach is treated rather shabbily in the American academic establishment.

This review will not assuage the author's view of American academia. When one thinks about economic reform, it is truly difficult to factor in the role of culture. Economic reform is in response to endemic economic problems that have been created by the design of an economic system known as the centrally planned economy. Indeed, if one looks beyond China, one is struck by how uniform the response has been from governments operating under vastly different cultural backgrounds. Russia and China, two countries whose cultural values have nothing in common, have adopted very similar reforms. This observation alone would cast doubt on the notion that culture is an independent variable that can account for the most important issue in contemporary political economy: Why did China or any other country undertake economic reform in the first place?

That said, the book is a thoughtful discussion of the role of values in Chinese economic policy and behavior. An example here is the discussion of the concept of "opportunity cost." The author argues that in Chinese traditional culture the concept of opportunity cost is present, but in a particular way. It is there not to calculate the benefits and the costs of a particular action but instead to measure the level of one's moral rectitude. To a Confucianist, an action that has high opportunity—and material—cost is all the more reason to engage in because it shows one's devotion to its moral cause. (Strictly speaking, the author here should mean just "cost" rather than opportunity cost. Opportunity cost is the foregone benefit associated with an action by not engaging in it, not the cost of the action that is being engaged in. Thus the opportunity cost of taking a moral action is whatever benefit one may derive from engaging in an immoral action.)

This theme of morality and its connections with economic actions threads through this book. The author argues and shows that China's socialist economy is at least consistent with the traditional emphasis on collectivity and group interests, if not directly shaped by it. Thus, to the extent that reforms move the economic system to a moral foundation drastically at odds with such an emphasis—toward individualism and material incentives—conflicts are bound to occur. Implicit in this approach is the idea that culture and the political implications associated with the changes in culture should belong to the left side of the equation rather than the right side. Indeed, the author is much more successful in demonstrating the cultural or moral effects of reforms than how the reforms have been shaped by cultural factors.

An example here is the introduction of the general manager system in Chinese factories discussed in chapter 6. The conventional argument for such a system is to more clearly assign responsibilities to the manager so that s/he has an incentive to improve efficiency. However, the author sees in this design the amoralization of the Chinese society. Under the previous system, the party secretary and the manager ran the factory jointly. This integration of "state and society," or of ideology and economic management, served the purpose of imparting empathy to and promoting selflessness among the workers. The old system was moral, while the new one is not. Another more extreme example appears in chapter 9 when the author discusses the potential political implications of basing one's actions so blatantly on profiteering motives even in an avowedly nonprofit institution such as a school.

The author provides more questions than answers about exactly what these political, cultural, or moral implications are. The author sees amoralization and cultural decay in a reforming and reformed China. This is a commonly held notion among Chinese intellectuals and like many other romantic notions they hold, this one is dead wrong. A market economy is infinitely more moral than a centrally planned economy because it at least has the potential to align the interests of individuals with the interests of the society, whereas the whole idea of a centrally planned economy is that the interests of individuals necessarily clash with those of the society. Planners, of course, value the interests of the latter more, and since there is no objective definition of societal interests, they simply assume that the interests of the society and their own interests are somehow equivalent. In the process, they proceed to impoverish the whole society by impoverishing each individual in it.

The author also sees challenges to the communist regime arising from these cultural changes because it is not serving as a moral example to its citizens. Here the author is right about the conclusion, but is probably wrong about the reasoning. The challenge to the communist regime will come from the same source as in many other countries undergoing democratic transitions: If the economic half of the Chinese population is increasingly able to make decisions based on autonomous interest and freedom, the political half will demand the right to do the same in due course. If the communist regime in China is to fail, it is to fail not because it is becoming untraditional but because it is too traditional in its current political form.

The Treasury and Whitehall: The Planning and Control of Public Expenditure, 1976–1993. By Colin Thain and Maurice Wright. Oxford: Clarendon Press, 1995. 580p. $98.00.

Paul Whiteley, *University of Sheffield*

This detailed study of policy failure has wider implications for new institutionalist theories of policymaking and also for debates about the control of public expenditure in advanced industrial societies. The authors have undertaken the most comprehensive and exhaustive analysis of the planning and control of public expenditure in Britain ever published. It covers a period of extraordinary political and institutional change, starting with the aftermath of the IMF crisis of 1976, through the election of Mrs. Thatcher's government, up to the point at which John Major consolidated his leadership of the Conservative Party by winning the general election of 1992. The machinery of government in Britain went through greater changes in this period than in any other comparable period this century. This study extensively examines the context within which policy is made, the institutions and organization of the system, the policymaking process, and expenditure outcomes during these years.

The dominant goal of the Treasury throughout the entire period was the control of public expenditure. Britain, along with virtually every other advanced industrial society, faces a fiscal crisis of the state, in which burgeoning public expenditure coupled with voter resistance to higher taxation is producing great political strains. Compared with the United States, Britain starts with a number of institutional advantages for coping with this problem. The list of these advantages is well known to students of the Westminster model: a very centralized unitary state, a vague constitution that can easily be manipulated, an executive that dominates Parliament, an ethos of Treasury dominance within the Whitehall bureaucracy, and, at least during the 1980s, a strong-willed Prime Minister with an ideological commitment to reducing the size of the fiscal state and who also had a long period of incumbency with which to do it. If the new institutionalists are correct, all these features of the state should make it easier to control spending, compared with a country like the United States.

Despite these advantages, the study is unequivocal in its conclusions: "Conservative Governments were unable to achieve either the heroic objectives set in 1980 or the less ambitious ones which replaced them in the mid-1980s" (pp. 439–40). As this statement suggests, the early period of the Thatcher government was characterized by the pursuit of ambitious goals involving both the reduction of government expenditure as a percentage of GDP and the scrapping of the neo-Keynesian ethos in the Treasury, with the aim of replacing it by monetarist macroeconomic policies—or, as the former Labour Chancellor Denis Healy later dubbed it, by "sado-monetarism." As the authors document very thoroughly, when these goals were unrealized, the government responded by moving the goalposts and settled for a reduction in the rate of growth of public expenditure, in addition to backing away from monetarism. However, the revised goals proved just as elusive as the earlier ones. Thus, the central finding of this book directly challenges the central proposition of the new institutionalism, the notion that institutions make a difference.

There is also strong support for the proposition, investigated some years ago by Richard Rose, that party incumbency makes very little difference to trends in public expenditure. The authors examined the growth in expenditure in different periods of incumbency, and a quick calculation from their Table 20.1 shows that since 1963 expenditure grew on average at 2.6% per annum during the 11 years of Labour incumbency and by 2.7% during the 21 years of Conservative incumbency. Governments, they point out, "have succeeded in cutting public expenditure in real or volume terms only at times of major external economic crisis, or during economic boom, and then only for limited periods" (p. 438). Thus, external factors produce within-incumbency effects but not between-incumbency effects. Again, institutions in the form of political parties appear to make no difference.

There are two weaknesses in Thain and Wright's analysis, one minor and the other more serious. The first is that too much space is devoted to investigating the institutional and organizational background to policymaking and to tracking the minute detail of the evolution of these policies over time. This means that they do not start to address the core question of whether or not the policy objectives were achieved until page 420 of the text. To be fair, they are describing complex and far-reaching changes, such as the introduction of the "next step" agencies, but quite a bit of this background is tangential to the overall aim of analyzing the relationship between objectives and outcomes, although political historians would no doubt disagree with this view.

The serious problem with this book is the lack of any theory of public expenditure that can be translated into a model and used to test propositions about the relationships between policy objectives and outcomes. All the conclusions described above are reached essentially by "eyeballing" data, so in the absence of a clearly defined model of the expenditure process we cannot be sure if the findings are valid. If the authors had been ambitious, they could have tried to model institutional and incumbency effects using the Treasury macroeconomic model, which can be accessed by researchers and has an extensive public expenditure sector. But even if this were regarded as beyond the scope of a political science approach, they could have done something simpler; there is a thirty-year-old research tradition in political science of the quantitative modeling of public expenditure, pioneered by Wildavsky and others. This approach can be utilized to specify some fairly simple models. Much recent econometric evidence suggests that public expenditure can be modeled parsimoniously as a random walk; thus, a simple ARIMA model, with various impact and adjustment coefficients for the effects they are describing, would have served its purpose as a first cut at modeling these processes. As it stands, they run the risk of telling the story in great detail while not being able to deliver a convincing punch line.

Apart from this important reservation, this is a book that ought to be read by all students of British politics, by new institutionalists, budgetary politics specialists, and students of the comparative economic policymaking process. It gives great insight into policymaking within the original Westminster model at a time when this is more under strain than ever before.

The Anti-Marcos Struggle: Personalistic Rule and Democratic Transition in the Philippines. By Mark R. Thompson. New Haven: Yale University Press, 1995. 258p. $32.50.

Gretchen Casper, *Texas A&M University*

People Power in the Philippines—when hundreds of thousands of Filipinos protected military rebels from loyalist troops, demanded that Ferdinand Marcos step down, and supported Corazon Aquino's claim to the presidency—has become a symbol of democratic transition. Several authors have presented detailed accounts of the collapse of the Marcos regime and in particular the dramatic events of February 22–25, 1986 (e.g., Arillo, Bonner, and Johnson). Rather than recounting the story, Thompson explains why the transition unfolded as it did and what effect it had on the new democracy. Furthermore, he offers a theoretical perspective by comparing the Philippines to other cases of regime change.

Thompson focuses on the behavior of the opposition to build an understanding of the Marcos regime, its collapse, and the legacies that hindered the Aquino administration. First, he shows that Marcos's personalistic rule caused the transition that resulted in his overthrow and exile rather than a negotiated settlement and continued influence. Second, the breakdown of the alliance between moderate and radical opposition groups led to a replacement transition in which the moderates had more influence than the radicals. Finally, the makeup of the opposition created the legacy of a diverse and fragmented coalition that threatened the stability of Aquino's government.

While other theorists, such as Crowther and Hawes, have considered the Marcos regime as bureaucratic-authoritarian based on its initial economic goals, Thompson characterizes it as personalistic (Linz's term is "sultanistic") and focuses on Marcos's desire to control power: "A sultanistic dictator exercises power not for a particular class but for the benefit of family and friends (p. 51)." Under such a regime, the ruler can solidify his power base while simultaneously eroding those of his political enemies through the widespread use of corruption. By considering Marcos's regime as personalistic, Thompson compares the Philippines with Cuba, the Dominican Republic, Iran, Nicaragua, Haiti, Romania, and Zaire (p. 4).

Thompson discusses the use of patron-client ties in pre-1972 politics to show that the Philippines' experience with democracy was clientelistic. However, when Marcos declared martial law in 1972, he dramatically increased the level of personalized power. Anti-Marcos politicians discovered that they had to join Marcos's party to receive any political spoils. Those members of the opposition who most threatened Marcos's rule, because of their independent power bases, found their economic (and, therefore, political) power diminished as Marcos and his cronies confiscated their corporations.

It is not surprising, given the Philippines' historical experience, that several politicians—including over half of the governors (p. 62)—switched sides and joined Marcos's party. However, many did not cooperate with the regime. Thompson spends most of the book discussing the opposition's efforts and organizations in great detail, mostly focusing on the moderates but also considering the radicals. When martial law ended in 1981 without any significant reduction in Marcos's extralegal powers, the moderate and radical opposition in the Philippines united. However, this united front did not last long; by 1982, the two sides had split apart and returned to their original strategies. The radicals focused on armed struggle and the moderates on victory at the polls (pp. 106–7). When the presidential election was held in 1986, Aquino and the moderate opposition were able to overthrow Marcos and gain predominance over the radicals.

Marcos held the presidential elections in 1986 one year earlier than constitutionally scheduled, hoping to shore up his support from the United States. He did not plan to lose the election: his position was one of "unwillingness to negotiate or hold free elections" (p. 9). However, he overestimated his ability to "win" the election convincingly. His intransigent position eroded any chance of bargaining with the other political actors. As a result, the Philippines' transition was brokered by the moderate opposition led by Aquino and the military rebels under Juan Ponce Enrile and Fidel Ramos.

Thompson points out that the Philippine transition is unusual, for two reasons. First, the most common type of transition results from bargaining in which the regime itself participates. Replacements, while dramatic, are relatively rare. Second, most personalistic regimes are overthrown by the radical opposition because their intransigence requires armed struggle to unseat them. In the Philippines, however, the Marcos regime was overthrown by the moderate opposition, supported by People Power (p. 6).

Aquino was able to gain the presidency because People Power made it clear to the rebels and to Marcos that she had the largest number of supporters. The rebels eventually agreed to drop their plans for installing a junta and to ally with her, and Marcos ceded the presidency. While a heterogeneous support coalition may have been necessary to overthrow a personalistic ruler like Marcos, the diversity of the coalition members' interests and levels of support for democracy threatened to undermine Aquino's administration. Once she was in office, Aquino had to mediate between coalition members with widely conflicting demands (p. 131). One result of this legacy was the wave of coup attempts against her administration by the military rebels.

This book is based on an impressive range of data that the author collected across five research trips to the Philippines from 1984 to 1992. Thompson interviewed over one hundred and fifty people, including many who were centrally involved in pre-1972 and post-1986 politics, as well as members of several opposition groups during the Marcos regime. He then augmented his interviews with information collected from archival research and the work of numerous Philippine scholars (such as Lande [1965] on patron-client ties in pre-1972 politics and Jones [1989] on the New People's Army). As with any book, readers may disagree with several points. (Was Pusyon Visaya an opposition party [p. 79]? Is the military's autonomy retained in bureaucratic-authoritarian regimes [p. 51]?) In general, however, Thompson's discussion of the Philippines is rich in contextual detail carefully collected from numerous sources.

Unfortunately, many readers will be less knowledgeable about the Philippines than Thompson is. Given this situation, it would have helped readers if the Philippines had been compared to other cases in a more systematic manner. Furthermore, by stressing how the Philippines is different from other personalistic regimes and replacement transitions, Thompson risks leaving the reader with the impression that the Philippines is an outlier case in an outlying pool. A more systematic comparison could have answered important questions from the Philippine case that also arise when studying transitions in other countries. For example, if replacement transitions are more likely to introduce significant political reform than are pacted transitions, why did the Philippines revert to pre-1972 politics while a revolutionary government was installed in Iran? With his focus on the behavior of opposition groups, Thompson could have built on the theories of such democratization scholars as Cohen and Przeworski that describe how alliances between groups affect the new government.

Thompson's book will be widely read by Philippine scholars because it is one of the first books to analyze the collapse of the Marcos regime using a theoretical and comparative perspective. Furthermore, his discussion is supported by an impressive body of evidence that draws on numerous sources to offer a highly detailed account of the role of the opposition in the installation of the Aquino government. For these same reasons, this book will also be read by comparativists who are looking for a detailed analysis of the Philippine transition case.

Intellectuals, Socialism and Dissent: The East German Opposition and Its Legacy. By John C. Torpey. Minneapolis: University of Minnesota Press, 1995. 320p. $21.95 paper.

Vladimir Tismaneanu, *University of Maryland at College Park*

A brief review may be unjust to John C. Torpey's comprehensive and well-researched analysis of the role of critical intellectuals in the collapse of the German Democratic Republic (GDR). His book details the tumultuous events of 1989 and their aftermath while making an outstanding contribution to the ongoing debate on the meaning of 1989, the legacies of Leninism, and the complex relationship between intellectuals and state socialism. Based on personal interviews with many of the various actors involved in East Germany's "tender revolution," this study may remain a definitive exploration of the illusions, utopianism, and failed expectations of those who represented, in Albert Hirschman's famous term, the "voice," the challenge from within to the rigid Stalinist dictatorship exerted by East Germany's Socialist Unity Party (SED) and its main instrument, the secret police (*Stasi*).

Torpey explores the independent movements and thinking under Leninist regimes, the ambivalence of the status of intellectuals under socialism, and the strange loyalty to the GDR, so intense and pervasive among that country's dissidents. One of the central merits of the book is Torpey's ability not to fall into the arguably tempting trap of emotional identification with the former dissidents. While they were the true heroes of the antibureaucratic resistance, they remained isolated from the East German population. Their ideal values, including the construction of an alternative form of socialism, were not at all shared by most of their fellow countrymen. For all their high moral ground (or maybe because of it), this tiny group of critical intellectuals was quite marginal within the old regime: most of the GDR intellectuals preferred accommodation and conformity. Some engaged in mild criticism of the SED, but they did not go beyond risk-free and oblique forms of dissociation from the rulers.

True dissidents were thus a marginal group, whose rise to political prominence in the fall of 1989 came as a surprise to them no less than to the power holders and to the population at large. They filled the political vacuum created by the regime's sudden breakdown, but their nebulous vision of an "authentic" socialist state had no political future once the issue of unification became a real possibility. The demise of the GDR was less the result of political opposition than of the consequence of mass exodus catalyzed by the dramatic changes that had taken place in other Warsaw Pact countries (e.g., Hungary, Poland). Once the SED's authoritarian domination was overthrown, there was no rationale for the existence of two separate German entities. This simple fact did not pass unnoticed by the Leninist ideologues, but it did not affect the dissidents' deep belief that a "noncapitalist" German state could still have a future. This political inadequacy of the critical intelligentsia explains the limited appeals of the new political formations that looked so promising during the hectic events of October–November 1989 (especially the New Forum).

The book is divided into six chapters that highlight the principal moments in the evolution of the East German critical intelligentsia. The first chapter is primarily historical and indicates the role of anti-Fascism as a main ingredient for the self-definition of the GDR intellectuals. Most of these writers, philosophers, and scientists accepted the official claim that the GDR represented a true anti-Fascist alternative to West Germany. This self-serving and self-deluding myth continued to inform and distort the GDR critical intelligentsia's perception of the real distinctions between the two Germanys. With very few exceptions, their dissent remained confined within the self-limiting paradigm of "socialism with a human face." Torpey lucidly argues that what mattered most for the East German anti-Stalinist intellectuals was the regeneration of socialism, not the real demands of the no-less-real working class. They were excited more by "revisionist" (humanist Marxist) movements in Poland, Hungary, and Czechoslovakia than by the workers' uprising in June 1953 in their own country. I agree with Torpey that this unrepentant leftist commitment may be one of the main sources of the continuous estrangement of the GDR critical intellectuals from their own population.

Chapter 2 explores the relationship between intellectuals, state, and opposition during the period inaugurated by Nikita Khrushchev's "Secret Speech" in February (not March, as Torpey writes) 1956, a real earthquake in the history of world communism, and ending in 1980, when Poland's Solidarity achieved legal status. Extremely important for understanding the mind of the East German critical intellectuals is the discussion of their enthusiastic endorsement of the Prague Spring of 1968. While in some countries (e.g., Poland and Czechoslovakia itself), the crushing of Alexander Dubcek's experiment in "socialism with a human face" led to the end of revisionist illusions, this was not the case in East Germany.

Chapter 3 examines the development of oppositional activities in the GDR in the 1980s, the disarray of the power elite confronted with Mikhail Gorbachev's policies of glasnost and perestroika, and the rise of independent peace and environmentalist movements from below. Students of civil society will find fascinating details regarding the strategic debates among East German dissidents, as well as thorough analysis of the role of religious institutions in encouraging such forms of dissent. Chapter 4 examines the most glorious, short-lived moment of euphoria in the life of East Germany's dissidents: the breakdown of the SED dictatorship and the emergence of new political movements championing the values of solidarity, trust, and political citizenship.

The last two chapters document the disappointments of the once-applauded heroes faced with the impact of nationalism, consumerism, and other trends that led to the end of the GDR and reunification. The author highlights the growing chasm between the former dissidents' search for a "solidaristic" third way, their desperate yearning to preserve East Germany's separate existence as a territory for a genuine experiment in building the "true" socialism, and the magnetizing, irresistible attraction exerted on the average East Germans by Western prosperity and freedoms. Not that the dissidents were not interested in human rights and political participation. But their agenda was dramatically marred by their refusal to admit that liberal democracy, as it existed in the West, was the most attractive proposition their fellow citizens were offered under the circumstances. The critical intelligentsia's talk about radical, "eco-pacifist" democracy and a "new politics" was doomed to fall on deaf ears, especially after decades of bureaucratic centralism obsessed with ideological purity. In Torpey's clear formulation, "The GDR's anti-Stalinist opposition was thus trapped between the inherited critique of capitalism, itself powerfully rooted in antifascism, and its achievements of 'bourgeois' civil rights; between its desire to overcome the dangerous division of Europe and its lack of enthusiasm about Western institutions" (p. 159).

Still, the question is what remains of the message of East Germany's dissident experience? Are the ideas of solidarity, tolerance, trust, and independent civic initiatives simple metaphors, expressions of poetic, rather than political imperatives? Torpey's answer is that the former critical intellectuals have continued to play a significant role in the reunited Germany. Their main objective is to influence the public sphere by promoting the notion of historical and politically sensitive citizenship and opposing the new waves of populist xenophobia in the name of the traditions of a humane and democratic community. Vividly written, informative, and superbly documented, this book gives us first-rate interpretations of not only the past, but also Germany's future.

Towards Consensus? The 1993 Election in New Zealand and the Transition to Proportional Representation. By Jack Vowles, Peter Aimer, Helena Catt, Jim Lamare, and Raymond Miller. Auckland: Auckland University Press, 1995. 253p. $29.95 paper.

Stephen I. Levine, *Victoria University of Wellington*

In November 1993 the New Zealand electorate voted to adopt a new electoral system for the country's triennial parliamentary elections. The referendum result meant that New Zealanders had chosen to replace the existing plurality or first-past-the-post system with a more proportional one derived largely from the electoral system in use in Germany during the postwar period. The vote on the electoral system was unusual for a number of reasons. First, New Zealand's tradition of parliamentary sovereignty mitigates against decision by referendum, notwithstanding an unbroken record of votes on a ballot question dealing with prohibition and state ownership alternatives to private enterprise control over liquor sales from 1919 through 1987. Second, as citizens of one of the few countries without a formal written constitution, New Zealanders tend to take a pragmatic approach to politics, and there is rarely much to be gained by politicians or political parties seeking to raise constitutional issues with the electorate. In this case, moreover, the decision to hold referendums—there were two, an "indicative" ballot in 1992, which revealed widespread support for proportional representation among those taking part, and the decisive 1993 vote—was taken by a government whose members were opposed to change and was backed by a parliamentary opposition equally unenthusiastic.

The referendum was held at the time of the general election and somewhat overshadowed it. Indeed, as the authors of *Towards Consensus?* emphasize, the 1993 election, won by the governing National Party with only a one-seat majority over opposition parties, gained in importance because of the vote for electoral change. In his election night victory speech and afterwards, Prime Minister Jim Bolger associated himself with the voters' endorsement of change, and his retention of office has given him and his party numerous opportunities to take the initiative in preparing for the first election (due no later than December 1996) under the new rules.

While political scientists interested in electoral system change have not altogether lost their professional detachment when viewing New Zealand, there is little doubt that the voters' choice has opened up opportunities for research, and these have been taken up with some enthusiasm. For comparativists as well as New Zealand-based scholars, the peaceful transition to the new electoral system (Mixed Member Proportional, or MMP) allows various propositions to be tested about causation and consequence. Research has already begun to examine a range of topics and themes, including the anticipated effects of MMP on Parliament, parties, and the public service; the implications for policymaking, the policy process, and coalition formation (the latter of particular relevance since New Zealand has had sixty years of single-party majority government); and the effects of the new system on voter loyalties and voting behavior.

The authors of *Towards Consensus?* are an experienced team of survey researchers, four of them on the staff of New Zealand universities and the fifth, Jim Lamare, an American academic with substantial New Zealand teaching and research experience. Helena Catt and Raymond Miller explored survey findings when interpreting two 1992 parliamentary by-elections in *Season of Discontent: By-elections and the Bolger Government* (1993), while Peter Aimer and Jack Vowles collaborated on a survey-based study of the 1990 New Zealand general election, *Voters' Vengeance: The 1990 Election in New Zealand and the Fate of the Fourth Labour Government* (1993), chronicling the collapse of an innovative but ultimately incoherent Labour administration. Like these earlier titles, the 1993 election book deploys survey evidence in an attempt to understand and explore the sources of voter movement between parties from one election to the next. What distinguishes *Toward Consensus?* is not its methodology or style or conclusions, but rather its subject matter, the conjunction of two voting decisions: the narrow election win for a free-market conservative government and the vote for a new electoral system designed explicitly to improve access to power for smaller parties and minority perspectives.

For New Zealanders familiar with the context and circumstances in which these two votes occurred, *Towards Consensus?* states its theme in the title. Many New Zealanders voted for MMP not because they understood how the system would work (survey research conducted since the referendum has shown persistently high levels of ignorance and misinformation), but because of a preference for a more cooperative, less adversarial style of politics. The question mark in the title raises the possibility that they are apt to be disappointed, as indeed many New Zealanders, uneasy about the fragmentation of political parties since the referendum, now suspect. Although the authors concede that the outcome of the electoral referendum makes that event "arguably of much greater importance to the future of New Zealand politics than the result of the general election" (p. 175), the book inevitably concentrates most of its attention on the relevance of survey evidence to voting at the election.

The authors carried out two surveys: one was a questionnaire mailed out to parliamentary candidates and party delegates; the other was a study of public attitudes and behavior, based on a postelection questionnaire sent out to two sample populations, one drawn from the electoral roll and a second from among those who had taken part in the 1990 study. Although there is little doubting the authors' passion for their subject, the narrative is a data-driven work, its momentum generated more by methodological than by political concerns. The 71 tables and 54 figures, and the appendices on statistical methods and research design (as well as the elector questionnaire), make for a scrupulously well-documented work, though not an especially exciting one. There are some lapses into jargon: voting for a third party, for instance, may be a sign of "behavioural displacement" (p. 136). Apart from an apparent sympathy for MMP, there are few expressions of personal preference, one exception being even more noticeable since it offers a rare recommendation and one that is contrary to the direction of the data: "Our findings do suggest that women candidates should be selected for other reasons than the simple pursuit of electoral advantage" (p. 165), a plea that endeavors to put to one side the unwelcome discovery of "negative effects of women candidates" (p. 165) in 1993.

As a postelection analysis of voting behavior, *Towards Consensus?* inevitably devotes much of its space to looking back at an event rather than forward to new developments. But political scientists interested in New Zealand electoral and parliamentary politics now write well within the lengthening shadow of the approaching MMP system. Already Parliament has changed its rules, its Standing Orders, in anticipation of new expectations and predicaments. Similarly, almost every chapter of this book concludes by looking ahead to the 1996 inaugural MMP contest, with speculation directed toward a range of themes, including voter volatility, expectations about the role of elected representatives, and attitudes toward "old" and "new politics" (p. 100) issues. Although at times fascinating for psephologists, the focus on survey data, the lack of historical depth, and the neglect of important sources make *Towards Consensus?* something other than a comprehensive analytical critique of New Zealand politics. There is by now a substantial literature on the behavior of New Zealand's voters and the reshaping of the New Zealand state over the past two decades. Scholars interested in considering propositions about a polity undergoing institutional transformation, against a background of rising public distrust of government, parties, and politicians, might well wish to familiarize themselves with some of that literature, as well as with the data-based inquiry found in this useful account.

The Waning of the Communist State: Economic Origins of Political Decline in China and Hungary. Edited by Andrew G. Walder. Berkeley: University of California Press, 1995. 280p. $40.00.

Yu-Shan Wu, *National Taiwan University*

Theories abound to explain the political change in communist countries at the turn of the decade. Among these, the modernization paradigm that emphasizes the decisive impact of economic and social change on politics is a popular approach. Its main theoretical problem, however, is how to ascertain the carrier of political change in communist countries. By definition, these countries lack the bourgeoisie class, who performed a leading role in political liberalization and democratization in the history of the West. In *The Waning of the Communist State: Economic Origins of Political Decline in China and Hungary,* edited by Andrew G. Walder, the authors attempt to solve the carrier problem by detailing the mechanisms of political decline caused by departures from the command economy.

Following Szelenyi's (1988) *Socialist Entrepreneurs* and *Remaking the Economic Institutions of Socialism* (1989), edited by Nee and Stark, Walder has collected eight case studies (five on China and three on Hungary) to show how a communist state would naturally decline in the process of structural economic reform. The thrust of their arguments is to identify the nascent private sector that emerged during the reform as the main carrier of political change in the two countries. This theme fits well into the modernization paradigm. It also resonates with the Marxist motto that "Whoever controls the means of production commands political power." Furthermore, it echoes the totalitarianism paradigm that once dominated the study of comparative communism in asserting that a monopoly on economic power is vital to communist rule and that departures from a command economy will bring political change in their wake. The Walder volume thus inherits the theoretical insights of modernization, Marxist, and totalitarianism paradigms and concretizes them with empirical cases.

The choice of China and Hungary makes sense, for these were the two communist countries that made really significant departures from the command economy to allow the emergence of a significant private sector. The cases thus suit the theoretical impetus of the volume. The richness of the eight empirical studies further adds to the strength of their argument. The discussion of the 1982 property rights reform in Hungary by Anna Seleny and Akos Rona-Tas shows how the state surrendered its exclusive control over ownership, production, employment, and resource allocation and how individual entrepreneurs in the second economy collaborated with state company managers to gain space of action, constitute a "subversive force," form their own political organizations to sustain economic reform, and ultimately establish a political party. The comparable chapters on China by David Wank, Ole Bruun, Yun-xiang Yan, and Dru Gladney demonstrate the decline of the Chinese state through a shift from "dependent clientelism" to "symbiotic clientelism," the emergence of an "informal hierarchy" alongside the state-dominated "formal hierarchy," the breakdown of the cadres' monopoly over resources, and the mobilization of ethnic identities among China's minorities to take advantage of the economic reform. In all four chapters, private businessmen are identified as the carrier of political change, through interactions with state bureaucrats and rural cadres. There is one difference between the Hungarian chapters and the Chinese chapters: the former deal with the whole private sector as created by the 1982 property rights reform, while the latter are case studies of localities (Xiamen, the Bin Shen district of Chengdu, Xiajia village in Heilongjiang, and a Muslim community near Quanzhou). Besides these six chapters, Shaoguang Wang and David Bartlett discuss fiscal and financial changes in China and Hungary, respectively, putting emphasis on local governments, workers, and foreign investors. Their discussions are on a macro level.

As a collection of valuable empirical case studies, the Walder volume is both rich in materials and stimulating in its findings. The major problem of the book, however, is the difficulty of making any generalizations beyond its immediate scope. Mainland China and Hungary are so unique in their economic reforms that it is difficult to apply any findings here to other socialist countries, let alone provide a general theory of change for communist regimes under economic reform. The Hungarian chapters and the Chinese chapters do not talk to each other at all, leaving Walder's introduction the only comparative discussion that one can find in the book. There is thus a lack of integration among the different papers, which is usually the case with collective works. This adds to the difficulty in coming up with any generalizations.

The political decline of the communist states is the core concept of the volume. However, it is ill-defined. Though there is no doubt that the Hungarian communist regime suffered from a failed economic reform and ultimately surrendered its political power, it can be argued that the Chinese communist regime has actually been strengthened through a very successful economic reform. One thus needs to operationalize "political decline" before being able to deal effectively with the subject. The authors of the eight empirical chapters, however, define the concept in widely different ways, such as the change of social attitude toward the ideology, the party's loss of control over labor, the rise of the second budget, the shift of the official trade union to an opposition role, the decline of entrepreneurs' dependence on officials for access to capital, the emergence of dual sources of values and aspirations, the change of the village cadres' role from "local emperors" to "prudent middlemen," and an increase in ethnic self-identities. It is indeed very difficult to argue that all the above represent different aspects of the same phenomenon of political decline.

Common sense demands that political survivability be included in the discussion of political change in communist countries. This aspect does not appear in the volume, as China and Hungary are lumped together to demonstrate that economic reform caused a similar decline of state in the two cases, without sufficiently recognizing that the two communist regimes have thus far taken divergent paths following their respective economic reforms. While one remains a stalwart defender of one-party dictatorship, the other has witnessed the collapse of communist rule and experienced six years of democracy and capitalist transformation. Political fate is not a serious concern of the volume, but neither is economic performance. The editor argues that political decline occurs independently of the relative success of reform. This being the case, one cannot include in the definition of political decline any factors that are intrinsically linked with economic performance, such as a higher degree of legitimacy derived from successful economic reform, even though these factors may be more significant in accounting for the political fate of a reforming communist regime.

This book is really not interested in the ultimate political outcome of economic reform. The introduction mentions that the decline in the organizational capacity of a communist regime does not lead ineluctably to any specific political outcome. What matters more may be a split in the party leadership, a coup, or a revolution. It seems to be the sole intention of the authors to establish a causal linkage between economic reform and political decline, however defined. In the final analysis, however, the book does not deal with what really caused the collapse or survival of communist regimes.

Socialist Unemployment: The Political Economy of Yugoslavia, 1945–1990. By Susan L. Woodward. Princeton: Princeton University Press, 1995. 443p. $55.00 cloth, $19.95 paper.

Valerie Bunce, *Cornell University*

This is a book that has been long in the making; not surprisingly, it is also long in the telling. The wait and its weight, however, are worth it. This is an important study, not just because of the arresting interpretations of postwar Yugoslav economic and political evolution, but also because of what it says about three issues of broader interest. One is the surprising similarities, as well as the more often-noted differences, between Yugoslavia and other socialist states. Another is the central role of elite ideas and governmental capacity in shaping the economic policies of states. The final issue is the considerable impact of the international system on the domestic affairs of small, vulnerable states with relatively open economies.

Woodward's concern in this book is with two interrelated issues. The first is explaining the causes and consequences of sizeable and long-term unemployment in the former Yugoslavia. This is of interest because a core tenet in the theory and practice of socialism is the abolition of unemployment. The second is using the issue of unemployment as a means of gaining insight into the evolution of socialist Yugoslavia—from the founding of the regime toward the end of World War II to its collapse in 1989–90.

Woodward's main argument can be put succinctly. Unemployment reflected the dissonance between elite ideology and elite capacity. Put more concretely, the Yugoslav leadership embraced from the beginning of their rule what Woodward terms the Slovene approach to building socialism. This emphasized, for example, a strong commitment to full employment, along with a preference for decentralized economic and political decision making, the development of light industrial goods, and a relatively open economy. The difficulty was that the application of the Slovene model to Yugoslav conditions worked to push levels of unemployment upward and, at the same time, to deprive the Yugoslav leadership of the institutional capacity necessary for effective management of the unemployment problem. The "culprit" responsible for unemployment, then, was not the "usual suspects"—for example, the values and interests of Yugoslav elites and their relations with each other or the strong incentives of firms within a self-managed economy to shed labor. Rather, it was the setting within which elites were forced to work—a setting featuring, for example, dramatic regional variations in the labor supply and levels of socioeconomic development and a highly interventionist international system. Indeed, it is the final factor—the international system of power and money and its powerful effects on Yugoslavia as a small country with an open economy, poised precariously between two political-economic blocs—that Woodward seems to see as the key cause of unemployment in socialist Yugoslavia and, it must be noted, as the key factor explaining a variety of other developments, including the eventual disintegration of the regime and the state.

In the course of developing her main argument about unemployment, Woodward makes a number of other original and, I would argue, convincing observations. Let me highlight a few. At the most general level, Woodward's analysis places economics, ideas, and international developments at the very center of the Yugoslav story. This is in direct contrast to many analysts who, especially of late with the disintegration of the state as well as the regime, have constructed the Yugoslav experience on the basis of wholely different variables—for example, nationalism, ethnic conflict, and the struggle among elites for political power. Woodward's book also parts company in other ways with many works on Yugoslavia. For example, she questions whether self-management is so important to understanding Yugoslav developments, whether the Yugoslav experiment is so different from the path taken by other reform socialist states, and whether there is not in fact continuity, rather than sharp disjunctures, over time in the politics, economics, and policies of postwar Yugoslavia. The final point is, in my view, among the most important arguments presented in the book, not just because of the impressive evidence the author presents in support of it, but also because of the ways in which this interpretation helps us make sense of the familiarity and the tragedy of Yugoslav economic and political developments during the 1980s. As Woodward summarizes: developments during this time seemed to have been a case of "politics as usual in unusual times" (p. 345).

Finally, there is Woodward's answer to one of the most obvious questions posed by the Yugoslav experience: How can we explain the failure of workers in that country to protest job insecurity by mobilizing in large numbers against the regime? Here Woodward uses a comparison with other liberalized socialist states, such as Poland, to argue that the strike activity of workers depends not just upon whether the working class is angry but also upon whether they are united. In the Yugoslav case, unity was undermined—by decentralized politics and economics, by regional variations in the economic setting, by the segmentation of the public and private spheres of economic activity, and by the failure of intellectuals and others to discuss the unemployment problem. The Slovene model and the sheer diversity of the Yugoslav landscape could thus be said to have contributed in important ways to the unemployment problem *and* to the inability of Yugoslav workers to become an effective political force in support of full employment.

Embedded in this book, therefore, are a number of important arguments, which, it must be emphasized, are accompanied by an impressive array of evidence. If there is one flaw with the book, it is the following. It is unclear in the final analysis whether Woodward is arguing that the international system was the source of many of Yugoslavia's problems, including unemployment, or whether the problem was in fact the decision to embrace (and to stick with) the Slovene model, a decision that could be said to have exaggerated both the power and the costs of regional socioeconomic diversity, as well as the destabilizing interventions of the international economic and political-military order.

International Relations

Under Northern Eyes: Latin American Studies and U.S. Hegemony in the Americas, 1898–1990. By Mark T. Berger. Bloomington: Indiana University Press, 1995. 588p. $35.00.

Eldon Kenworthy, *Whitman College*

Mark Berger traces the "profoundly complementary relationship between Latin American specialists and the US imperial state" (p. 231), alternating summaries of U.S. policy toward that region with summaries of the interpretations offered by influential academics at the time. "Since the early twentieth century," Berger concludes, "US foreign policy has flowed from a commitment to a powerful liberal teleology which understands history as progress toward liberal capitalist development" (p. 231). The dominant discourse among U.S. Latin Americanists was and remains liberal. Even when influential Latin Americanists criticize U.S. foreign policy, as they did in the Reagan era, they "domesticate" the more fundamental criticism of the left, thereby marginalizing leftist scholars and obscuring the policy's true sources within capital accumulation. By these means, the dominant professional discourse on Latin America contributes to "the strength and resilience of US hegemony"; it is "central to US efforts to manage Latin America" (pp. 21, 228).

Its encyclopedic quality makes this a valuable book, whether or not one accepts its overall interpretation. More than half the book consists of notes and bibliography. Within the narrative half, the author not only identifies "liberal," "conservative," and "radical" Latin Americanists but devotes whole pages to exemplars of each tendency, tracing their careers and publications. This process is repeated for diplomatic history, development economics, and political science in each of several epochs. As a result, the documentation is massive, while the representations of individual texts and policies seem plausible as well as informative. The literature reviewed, of course, far exceeds what this reviewer has read. While making disclaimers, let me also add that I appear in this book as a liberal Latin Americanist allied with radicals during the Reagan period.

The potential scope of his project led the Australian author to limit his purview in two ways. While events as late as 1994 appear in the narrative, this history ends with the defeat of the Sandinistas in Nicaragua's 1990 elections, a moment when much scholarly attention was paid to "transitions to democracy" throughout Latin America. Similarly, while generalizations to all of Latin America and to all U.S. Latin Americanists abound, Berger's focus is on Central America and the Caribbean. In his preface and introduction he acknowledges these as choices made to keep the project manageable. Troubling is his failure to consider the impact such choices may have had on his conclusions.

Allowing Central America to overrepresent Latin America stacks the deck in favor of interpretations that emphasize U.S. hegemony. Ending with 1990 leaves many influential area specialists *seeming to* talk about the same things as the policymakers. Thus Berger concludes that the dissent from foreign policies that dominated the Latin Americanist discourse from 1968 on—accelerating during the Reagan eighties—was atypical. By 1990 the liberal consensus has been restored and we are left with scholars once more promoting the liberal teleology that supports the state's "managerial approach" to Latin America (p. 185).

Major academic conferences and journals of the past decade have not privileged, however, a managerial approach to "development" and "democracy," nor have they expressed confidence in Latin America fulfilling idealized U.S. values. On the contrary, the attention paid social movements, identity politics, and environmental constraints expresses many U.S. Latin Americanists' skepticism regarding the replicability or desirability of the U.S. model. Witness the outpouring of writing on the human and environmental devastation wrought by the neo-liberal economic policies backed by U.S. administrations from Reagan's to Clinton's.

Abetted by electronic communications and encouraged by the Latin American Studies Association (LASA), this professional discourse has become less bounded by nationality and by academic discipline to the point where it no longer is clear that it is controlled by U.S. scholars or institutions. Over a thousand of those attending LASA's 1995 conference, for instance, came from outside the United States. Now, as before, any U.S. administration can find Latin Americanists to legitimate its policies and write its reports, but Berger's claims deal with central tendencies, not minority representations.

So an interpretation that fits the first two-thirds of the century grows procrustean as it wrestles with the final third. As a recognizable field of inquiry, Latin American studies *was* the conscious creation of government agencies allied with private foundations and research centers working with compatible scholars and journals. Dana Munro and Leo Rowe exemplify a pre–World War II pattern carried into the Cold War era by the likes of Robert Alexander. Once we move into the 1970s, however, maintaining a collusive interpretation of *central* tendencies in the discourse induces Berger to perform contortions. How are we to categorize a Richard Fagen or a Walter LaFeber? Are they "liberals" or "radicals"? Liberals "domesticating" radical insights from positions of privilege within major universities is Berger's take. To see in dependency theory a "reinvention of liberalism" (p. 102) is just one of the stretches Berger makes to save his thesis.

If a scholar chooses civil society or sustainable development as the focus of her research, does that mean her work necessarily supports U.S. policies that also lay claim to such values? Does an understanding of "the historical and political determinants of knowledge" render it useless to seek objectivity in scholarship or to criticize its absence in government documents (pp. 9, 209)? Berger answers yes to both questions, ignoring the power that would be handed to the state by default were all scholars to follow his lead.

Despite viewing discourse as politically contingent, Berger has little empathy for U.S. scholars reclaiming liberal values to forge coalitions that may affect policies. Nor does he show any interest in the rethinking of leftist positions post-1989 as exemplified by Jorge Castañeda's (1993) *Utopia Unarmed*, a book listed in his bibliography. Are we not seeing as many "radicals" revisiting "liberal" concerns as "liberals" incorporating "radical" ones? What do these labels really mean in the work of observant Latin Americanists?

Overarching interpretation aside, Mark Berger has provided a valuable guide to the social science and historical literatures within Latin American studies as those have evolved in the United States.

Clinton and Post–Cold War Defense. Edited by Stephen J. Cimbala. Westport, CT: Praeger, 1996. 197p. $55.00.

Robert J. Lieber, *Georgetown University*

Foreign policy played a minimal role in the 1992 presidential election, and the Clinton administration came to office in January 1993 with only the rudiments of a foreign and defense policy. Moreover, the new administration confronted the task of adapting American strategy and military forces to a dramatically changed post–Cold War environment. The Bush administration had begun the task, but much remained to be done when the first Democratic president in twelve years entered the White House.

In a serious effort to analyze the Clinton administration's post–Cold War defense policy, Stephen Cimbala and his collaborators address a wide range of related topics. These include an overview of the Clinton defense program, defense budgets, planning, nuclear weapons, alliance management, peacekeeping, low-intensity conflict, the defense policy process, and civil-military relations. The evolving nature of the post–Cold War period and the new kinds of problems it has brought for policymakers make it difficult for any group of authors to derive a coherent and focused picture or fully to agree among themselves. The differences among the authors themselves to some extent reflect this reality. Nonetheless, their work displays the not-inconsiderable virtues of serious and informed analysis, a willingness to draw the logical conclusions from each of their analyses, and a welcome degree of readability.

Among the most valuable essays are those dealing with defense budget and force structure. Don M. Snider and Andrew J. Kelly identify what they believe to be a mismatch between defense strategy, force structure, and budgets. While they do not see an immediate problem facing the United States, they express concern over Clinton cuts in defense spending and identify a possible future in which, over the course of a decade, the United States may find that it lacks the capacity to deal with certain plausible threats. By contrast, the chapters by Lawrence Korb and Paul Davis provide different perspectives. Korb notes that Clinton found it necessary to make larger-than-anticipated defense cuts in order to deal with the overall budget deficit at a time when defense represented more than half the discretionary spending in the federal budget, though he observes that the United States continues to spend as much on defense as the next ten countries combined. He also is critical of the way the late Secretary of Defense Les Aspin managed the important Bottom-Up Review (BUR).

Paul Davis, in turn, sets out a more positive view of the combined changes in defense policy carried out by the Bush and Clinton administrations. These include producing cuts of some 30–40% in forces and budgets over the course of a decade, and Davis argues that these reductions have produced a relatively sound strategy for the post–Cold War environment.

Several of the authors address the related subjects of U.S. military intervention and involvement in UN peacekeeping. Donald Snow provides a sober treatment of the difficulties of intervening in conflicts caused by ethnicity or the consequences of failed states, and he points to the legacy of Colin Powell in fostering restraint toward foreign intervention. He also laments the insistence of the U.S. government on calling wars "peace operations" and asks, "Did George Orwell have a hand in all this? Have we fallen down the rabbit hole and landed in Wonderland" (p. 100). In turn, Sam Sarkesian describes the United States as remaining "ill disposed and ill prepared to respond to unconventional conflicts" (p. 118).

Stephen Cimbala provides a valuable treatment of Clinton policy toward UN peacekeeping, though he covers some of the same ground as Snow and Sarkesian. In doing so, he assesses the criteria developed by the Clinton administration and its predecessors in an effort to develop concrete and sometimes overlapping guidelines for judging when and where the United States should be prepared to intervene militarily.

Only one essay focuses squarely on the topic of nuclear weapons policy. This in itself is a sign of the very different strategic environment produced by the end of the Cold War. Nonetheless, the subject remains critical, and William Arkin and Michael Mazarr have written an essay that is unsparing in its criticism of the Clinton administration for what they regard as its failure to deal more effectively with the urgent issues of proliferation and nuclear arms control. Indeed, they describe a yearlong review of nuclear weapons policy by the Department of Defense as having produced "an uninspired document that made virtually no significant policy changes and left arms control specialists speechless with disappointment" (p. 49).

Though the essays themselves adopt varying perspectives and reach different conclusions about Clinton defense policy, there is much here that will be of value to those who wish to gain insight into and understanding of this fluid period in which American military strategy and force structure are being adapted to the post–Cold War world. Inevitably, there are some limitations. Several of the essays cover similar ground on issues of military intervention, and—inevitably—a number of the references appear dated. There is some sense of this in Lawrence Korb's account of Les Aspin's difficulties at the Department of Defense (with emphasis on the troubled BUR exercise) and in Linda Brady's otherwise insightful treatment of alliance management (which has been overtaken by more recent events in Bosnia). In reality the book covers just the first two years of the Clinton period—a time when the difficulties of the new administration were particularly marked.

Finally, for students of the presidency and foreign policy Cimbala's conclusion to the book provides his epitaph to the tumultuous first half of the Clinton administration by praising with faint damns: "The Clinton administration's first years leave behind a heritage of partial accomplishment and unintended dissonance arguably no worse than some of its Cold War predecessors."

In all, this remains a very useful (albeit overpriced) collection, which is of the quality to take its place among such serious work on strategy and defense policy as those published by the IISS in London, RAND, and the Mershon Center at Ohio State.

The Macedonian Conflict: Ethnic Nationalism in a Transnational World. By Loring M. Danforth. Princeton: Princeton University Press, 1995. 273p. $29.95.

Adamantia Pollis, *New School for Social Research*

This volume is a much-needed scholarly study of a little-known people—the Macedonians. Of all the new states that have been formed after the disintegration of Yugoslavia, the Republic of Macedonia has received the least attention by the media, but more important, it has been neglected by social scientists. The violence, ethnic cleansing, and genocide that erupted in Croatia, Serbia, and Bosnia-Herzegovina have not plagued Macedonia. Only the bitter, still unresolved, nationalistic dispute with Greece over its name has made headlines.

The author, an anthropologist, analyzes the conflict between Greece and Macedonia within the framework of ethnic and national identity. In so doing, he makes it clear that whereas ethnic and national identities have merged in Greece, among the Macedonians identities are fluid and in the process of being constructed and reconstructed and have not been consolidated. Nevertheless, there is an official Macedonian nationalist identity that the author contrasts at the outset with the Greek one. He explains that both states lay claim to the same historical legacy dating from the time of Alexander the Great, including the name *Macedonia* and the same symbols (the Vergina star/sun) and heroes. Moreover, despite the fact that ethnic identities, in particular their transformation into national identities, are historical constructions (in the case of Macedonians of recent vintage), they both justify their claims in terms of primordial ethnicity. In Macedonia, however, nationalist ideology still includes strong advocates of a united Macedonia, the VMRO.DPMNE, which would include Greek and Bulgarian Macedonia, an irredentist movement Greece went through in the early twentieth century.

The author spends considerable time discussing the "Macedonians" in northern Greece—not the geographic area, but people whose self-designation is Macedonian. He rightly argues and documents that Greece, being an ethnonational state, rejects the existence of ethnic minorities within its borders, except for the Muslim/Turkish minority dating back to the Treaty of Lausanne in 1923. The Macedonians of northern Greece, some of whom in the past designated themselves as Bulgarians, are identifiable largely by their language, which is a Slavic dialect of Macedonian. Nevertheless, he makes it clear that people in this linguistic category have assumed different national identities—some Greek, some Macedonian, some ambivalent or confused. Their harassment by the Greek authorities, including denial of legal status for their cultural clubs, prohibition against the use of their language in church services, and discrimination in employment, is extensively documented. Moreover, the author discusses at length the activities of a contemporary Macedonian human rights movement in Greece that struggles for the rights of this minority.

One of the author's significant contributions to the literature on ethnic nationalism is the global context in which he places the national communities. Diaspora communities, both Greek and Macedonian, in pluralist societies such as Canada, Australia, and the United States, constitute a major political force in propagating their respective national identities and furthering the national interests of their home state. Concentrating on field work conducted in Australia, with some material from the United States, he exhaustively documents the many Greek and Macedonian ethnic associations. Not only are these groups advocates and supporters of their respective states, or of their minority rights in states where they are not the dominant group, but they also serve as pressure groups in international fora.

In his discussion of the Macedonian diaspora, Danforth rightfully points out that for understandable reasons they are less well organized and powerful than their Greek counterparts. Furthermore, the lack of unity regarding the premises of a Macedonian nation are apparent in the diversity of goals among the various ethnic organizations. Most important, however, the author, through the narratives of numerous individuals, discusses identity transformation in Australia at the individual level from Greek to Macedonian among people who immigrated from Greek Macedonia. Unfortunately, most of these cases are presented on the personal, psychological level and do not address the broader social or economic setting that may have led to the construction of a new identity. Of especial significance is the role of old or new elites in particular social settings in mobilizing constituents. Nor does the reader get a sense of how widespread this transformation has been.

This book is a significant contribution both theoretically to the study of ethnic and national identity and specifically to those interested in Balkan politics. Its theoretical contribution is twofold. First, Danforth's emphasis on transnational communities and the role of the diaspora adds a dimension hitherto lacking in theories of nationalism. Second, he challenges the prevailing view that a shared culture is critical for ethnic identity. Furthermore, this is a much-needed study of one neglected state in the Balkans. The reader only wishes that he had included a brief discussion of the large Albanian minority in Macedonia. Such a discussion would have provided insight into whether Macedonia will in time become an ethnonationalist state, as all the other Balkan states are or aspire to be, or whether it will become pluralist.

One difficulty with this volume is that it does not flow smoothly. It is as if each chapter was written separately, with the result that there is often unneccesary repetition. As a result, the main argument occasionally gets sidetracked. Moreover, perhaps in the hope of strengthening his argument, he at times provides too much detail, such as the listing of all diaspora associations, which can detract from the very point he is making. Finally, it would have been useful for a better understanding of the Balkans if more had been included on both the politics and the economics of Macedonia and Greece as it pertains to ethnic and national identity. Despite these limitations, this book is a monumental study that raises important theoretical and empirical issues for further investigation and analysis.

Can Europe Work? Germany and the Reconstruction of Postcommunist Societies. Edited by Stephen E. Hanson and Wilfried Spohn. Seattle: University of Washington Press, 1996. 248p. $35.00 cloth, $17.50 paper.

Daniel N. Nelson, *Old Dominion University*

By mid-1993, postcommunist transformations were beginning to look as if the climb out of Leninist authoritarianism might be longer and steeper than first imagined. War had spread in the former Yugoslavia, and signs of rising public discontent with the first echelon of postcommunist governments were evident. Germany's reunification, then still very recent, was nevertheless being reevaluated, as violent attacks on Turks, Roma, and other minorities continued and Helmut Kohl's newfound foreign policy assertiveness was getting on the nerves of some NATO allies.

Four years later, conference volumes with their origins in 1993 are inevitably dated. Personalities have come and gone, causes have waxed and waned, and still within the first decade since communist parties abandoned power in Europe the notion of "reconstruction" has faded. Where once a leitmotif of anticommunism unified and propelled peoples from Warsaw to Sofia, relabeled and reformed communists now rule throughout the region (except in the

Czech Republic). Free and fair elections have returned younger *nomenklatura* from the last years of communist rule to parliamentary and governmental leadership. Fraudulent elections accompanied by substantial police violence have been the hallmarks of anticommunist leaders such as Sali Berisha in Albania to thwart the electoral expression of the population.

That a short span of time could make so much difference in how one describes the political landscape of Central, Eastern and post-Soviet Europe suggests the need to regroup around theoretically important propositions. Rather than assessing how the West (or specifically Germany) might "reconstruct" erstwhile communist systems, we must now consider why fears of a market economy are so deep and why the appeal of authoritarian practices remains so strong. And the more specific cases, such as the Czech Republic, remain exceptional, the more important become broader explanatory models that can account for such variation.

When planning the 1993 conference that led to the Hanson and Spohn volume, the University of Washington's Jackson School clearly assumed that Europe could indeed "work" after the Cold War. Constraining nationalism, channeling Germany's enormous potential within multilateral frameworks, and quickening the pace of socioeconomic integration constituted principal challenges before Europe could function truly as one. Embedded in the Hanson and Spohn book, however, was the assumption of synergy between economic and political transformations. With the return of privatized property, economic growth, moderated inflation and Western-oriented trade, a healthy civil society and other benefits of a "free market democracy" were expected to trickle down to all as Europe's civil space diffused eastward.

But this rosy scenario happened nowhere. For some politicians and for civil society generally, substantial political costs accrued to those who most favored rapid socioeconomic transformations (e.g., Mazowiecki and Balcerowicz in Poland, Gaidar in Russia). The costs, conflicts and crises of postcommunism do not make much of an explicit appearance in a volume such as *Can Europe Work?*

In a broader sense, however, Hanson and Spohn have assembled a volume about obstacles that lay ahead in Europe's future. Groups of chapters on nationalism and religion, on Germany, and on socioeconomic transitions comprise the 3-books-in-1 format. There is little integration among the volume's three sections, and relatively minimal focus on Germany notwithstanding the book's subtitle. Linkages can, however, be drawn by readers insofar as all chapters consider impediments on the way to a "Europe whole and free"—which, from the latter half of the century's last decade, seems to have been even more vacuous than most slogans.

Among the contributions are several useful studies and insightful essays. Economic historian Ivan Berend, for example, offers a probing analysis of the German economic role in East-Central Europe, pointing accurately (in 1993) to Germany's rapid reassertion of regional trade and investment dominance. Four years later, the German position is greater still, contributing in part to desires for NATO and EU expansion among Baltic-to-Bosporous corridor states—a goal too often presumed to be directed solely against an irredentist-drive Russia of the future. Concerns in Poland and the Czech Republic about long-term consequences of such financial control have also risen since Berend wrote his chapter. Also of particular note is Wilfried Spohn's own lengthy essay on the "German question," in which he skillfully interweaves the voluminous literature on the issue while emphasizing that Germany now exhibits traits—transnational integration and federal decentralization—that substantially alter Germans' place in Europe vis-à-vis in earlier epochs. Several other chapters are much less well developed and could be best termed "thought pieces."

Can Europe Work?, however, does not take us very far in analytical terms and does not join the larger debate about European integration and architectures. Dealing with nationalism, integrating Germany, and ensuring economic and political transitions in Eastern Europe looked like sure bets to be linchpins of Europe's future from the standpoint of only a few years ago. Instead, we see Europe undergoing wrenching debates about deepening *within* the EU (at the Turin Inter-Governmental Conference), where the issue is sovereignty vis-à-vis Brussels, not nationalism directed against minorities, and where too much EU power, not German engagement, is at issue. To the east, we see insecurity wrought by market transitions buffeting nascent democratic procedures and giving rebirth to demagogic politics.

One of the contributors to Hanson and Spohn's volume, Daniel Chirot, writes in his introductory essay that "more will be required" of Central and Eastern Europeans for their rebuilding to succeed. But, where only a few years ago the process of joining postcommunist Europe to prosperous democracies seemed to necessitate "loyalty to an idea" and "determination to sacrifice," those traits are certainly insufficient. Four years later, sacrifice or ideals cannot explain the "Czech exception" that remained, albeit shaken, after the 1996 elections. Instead, prosperous and protected, Czechs can view the future with a security that provides the milieu in which democracies can be nurtured.

Absent the diffusion of security, a democratic and prosperous Europe is unlikely to extend much beyond today's NATO and European Union. Such a simple but powerful message could have and should have played a larger role in *Can Europe Work?* Chirot notes correctly that stock terms like *democratization* and *privatization* will not suffice, but he falls back on the need for a "triumph of liberal values" as his key to achieving all goals of an integrated Europe. However, Hanson and Spohn neither offer nor derive any hypotheses as to what *will* suffice, that is, what NATO, the EU, and the United States must do (and why) to avoid catastrophic failure. As it stands, Hanson and Spohn have aimed broadly, succeeding in tantalizing readers with provocative issues. *Can Europe Work?*, however, gives us little to build upon as we now struggle to explain the volatile political environments in postcommunist states.

Pan-Turkism: From Irredentism to Cooperation. By Jacob M. Landau. Bloomington: Indiana University Press, 1995. 256p. $14.95 paper.

Frank Tachau, *University of Illinois at Chicago*

This is an expanded and updated version of a study originally published in 1981. Given the heightened interest in the Asian republics of the former Soviet Union, it is a timely and informative contribution. The author, Professor of Political Science at the Hebrew University, traces the development of national identity among the Turkic peoples of these republics and their relations with the Russian state, later the Soviet Union, and also with the Turkish nationalists of the late Ottoman Empire and the Republic of Turkey.

Landau has left few stones unturned. The account is based on source materials in at least six languages (including Turkish, Arabic, and Russian) and is heavily footnoted (72 of the 234 pages of text are taken up by footnotes, and the bibliography comprises another 25 pages). Thus, at the very least the book is a guide to a very wide variety of documents and publications rarely if ever gathered in one place in a format accessible to Western readers.

The story of the rise of national consciousness among Turkic groups in southern Russia and Central Asia is reminiscent of Benedict Anderson's concept of "imagined communities," particularly with respect to the emphasis on journalism in vernacular tongues. Virtually all of the intellectuals and writers cited here chose journalistic outlets to set forth their ideas and arguments and to idealize Turkic and/or Turkish literature and folklore. This case is complicated, however, by the distinction between nationalists in the Russian sphere and those in the dying Ottoman Empire. The former of necessity advocated autonomy within or separation from an existing state and were highly vulnerable to interference and repression by the government of that state. By contrast, the latter were in control of an existing state, albeit one with a highly uncertain future. Their first priority was preservation of that state. It was only as a last resort, as Landau notes, that they adopted an ethnic-based concept of national identity. Ironically, this development occurred at the same time that the Russian state was also paralyzed by revolutionary upheaval. The shift to an ethnic definition of national identity in the defunct Ottoman Empire gave rise to the contemporary Republic of Turkey. Simultaneously, the window of opportunity seemed to open up for full development of the ambitions of Turkic nationalists in Russia. But just as the Republic of Turkey took shape in the early 1920s, the consolidation of the USSR slammed shut the window of political opportunity for ethnic Turkic nationalism in Russia.

Professor Landau draws a sharp distinction between what he calls Turkish nationalism and pan-Turkism. The former refers to the territorially constrained nationalism adopted by the Turkish Republic, the latter to a more expansive version of nationalism including all Turks or Turkic groups residing outside the boundaries of the Republic. In fact, however, his account clearly shows that there was a great deal of cross-fertilization between these two versions of nationalism. Indeed, although overt advocacy of political action based on pan-Turkist ideas was frowned upon by the Turkish government throughout most of its history, the official history texts developed for use in Turkish schools in the 1930s contained unmistakable overtones of pan-Turkism. Moreover, "outside" Turks (i.e., emigrants from Russia) played leading roles in propounding and advocating concepts of ethnic nationalism during the transition from the Ottoman Empire to the Turkish Republic.

Landau's account strongly implies that *raison d'etat* determined which version of nationalism was to prevail and even the terms of public debate on the issue, at least inside Turkey. Thus, for most of the time between 1923 and 1990, overt advocacy of pan-Turkism was not tolerated. The only exception was a brief period during World War II when it appeared that Nazi Germany might conquer the USSR. It was also at this time that racist ideas came to the fore among advocates of pan-Turkism, mirroring the racist bias of Nazism. But both the Nazis and the Turkish racists overplayed their hands, and a crackdown on the activities of the latter quickly ensued.

In more recent years, Landau suggests, pan-Turkism has entered the mainstream of Turkish politics, specifically in the form of the Nationalist Action Party led by former Colonel Alpaslan Türkes. But Landau further shows that, as this party has shouldered advocates of militant pan-Turkism off the political stage, its leader, an avowed pan-Turkist in the past, has increasingly diluted his support for such ideas in favor of maximizing the electoral appeal of his party. Furthermore, the unexpected achievement of independence by the Central Asian republics has not produced much more than expressions of fraternal feelings of solidarity, along with the competitive pursuit of new business opportunities. These republics have proved too jealous of their new status and too uncertain in their relationships with Russia and with sizable Russian minorities in their midst to entertain radical ideas of reorientation or reorganization on the basis of ethnic sentiments alone.

Professor Landau has provided an up-to-date compendium of facts concerning the history of these nationalist ideas and movements. Students of nationalism in general and of the politics of post-Soviet Central Asia and the Turkish Republic in particular will remain greatly indebted to him for some considerable time.

Beyond Westphalia? State Sovereignty and International Intervention. Edited by Gene M. Lyons and Michael Mastanduno. Baltimore: Johns Hopkins University Press, 1995. 324p. $48.50 hardcover, $16.95 paperback.

Daniel J. Whiteneck, *United States Air Force Academy*

Can scholars and students of international relations and world politics concentrate their studies on a different set of theoretical questions than those that were preeminent from 1648 until the end of the Cold War? This book does an excellent job of raising that very issue, with significant contributions from case studies and, more important, interesting theoretical essays.

The editors, Gene Lyons and Michael Mastanduno, frame the issues in terms of the "rights" of the international community to intervene without the approval of a sovereign state, whether or not there is a right to intervene emerging in the name of the international community's values and norms, and whether or not growing interdependence has shifted the balance between the rights of states and the authority of the international community. The theoretical issues that flow from this are: can the international community be a purposeful political actor?; under what circumstances does the community decide to intervene?; where does the authority to intervene come from?; and what are the conditions that make intervention legitimate to the international community? These are all good questions. They seek to define sovereignty in an interdependent world, expand the study of intervention beyond power dimensions to include norms and values, and provide some assessment of critical cases that could demonstrate a decisive break from the Westphalia past. This is accomplished in a way that moves us beyond the narrow legalisms of international law literature or an overreliance on the forms and functions of international organizations. The authors move past these by exploring sovereignty and legitimacy and intervention as political ideas that are contested by actors, both state and nonstate, in the international arena. The actual international legal environment and the actions of international organizations are the results of these contests.

A key question that places these concepts in the context of the historical development of world politics is, however, not addressed. This issue is not whether intervention moves the system beyond Westphalia, but to recognize that intervention has been a long-standing practice in world politics and to examine how the ideas that have motivated intervention by the international community have changed over time.

Friedrich Kratochwil, Nicholas Onuf, and Robert Jackson provide useful insights into the core concepts that are at stake in this study. Kratochwil and Onuf do not break new ground when they ground sovereignty as an institution in a web of societal practices. What they do do, however, is to implicitly concede that there is an international society that is well formed enough to create such strong institutions. This raises the possibility for further research into the creation of the institution, its limits, and its strength vis-à-vis the international society at various historical periods in world politics. Then sovereignty would be a contested concept since 1648, and its strength as an institution would be contingent upon the state of international society. The contingency could be a function of the distribution of power in world politics, the degree of interdependence among actors, the development of societal norms and values, or the coordinating capacity of international organizations, both formal and informal. Research along these lines is indicated from the concepts outlined here.

The cases in this volume are uneven in their ability to shine new light on the larger issues the editors want to address. Jack Donnelly ("State Sovereignty and International Intervention: The Case of Human Rights") avoids the dichotomy of statist or cosmopolitan models of the world polity by creating a third model that he labels "internationalist," or a world society made up of the formal and informal practices among states that are the society's members. He does not blur the lines between the three, making them more distinct than a continuum, with absolute state sovereignty on one end and a global society of individuals with international rights and powerful international organizations on the other end.

Thomas Weiss and Jarat Chopra ("Sovereignty Under Siege: From Intervention to Humanitarian Space") also address human rights questions. They concentrate on the tension between sovereignty and other values, the blurring of the line between domestic and international issues, and the need to distinguish typologies of interventions that would safeguard lives.

Both of these articles by necessity force research on the theoretical and empirical levels into definitions of legitimacy, power politics, and human rights, as well as the nature of the rights of individuals in international law. It also begs for a closer examination of state interactions, perceptions, and expectations to determine both the norms, values, and actions that constitute social practices in the global polity and also just when a state has violated those practices to such an extent that intervention by the rest of the community would be legitimate.

The articles by Ken Conca on environmental protection and intervention and by Janne Nolan on intervention to control the proliferation of weapons of mass destruction point out the difficulties of answering any of the above questions positively in the near future. Both authors give due weight to the current and future problems posed by the power disparities between states of the North and the South, and between the disparities in interests on these crucial issues between rich and poor countries. It is this power dimension that is missing from the first two cases, and it is power that has driven intervention in the past and is likely to do so in the future on security issues related to topics like proliferation.

Closing essays by James Rosenau and Stephen Krasner point to some interesting areas for further study. Rosenau argues that changes in the principle of sovereignty must be measured by changes in the behavior of international actors. Sovereignty becomes a continuous variable that is changed by situational, domestic, international, and legal determinants toward either pole. Rosenau assesses such movement as "halting and punctuated," but movement is occurring toward the interventionist pole. Such conclusions argue for the use of evolutionary models from the various social sciences to study this movement. Such punctuated equilibria models could provide valuable insights into the characteristics of periods of greater or lesser intervention. Evolutionary models can also help measure large-scale changes in global processes—political, economic, and social—that affect state behavior.

A longer time frame is exactly what Stephen Krasner provides with his reminders that the tension between sovereignty and intervention have a distinguished history. He is correct in noting that realist theories of independent state actors and state autonomy have been eroded by a history of interventions for a variety of reasons. He is also right in arguing that liberal interdependence, by concentrating on a systemic transformation of actors, attitudes, and interests, does not account for the continuing importance of power and direction of the global polity in determining future interventions. An answer to these problems lies in an examination of the changing nature of global problems and the creation of a legitimate coalition of states that have the power to act.

This book has some new ideas about sovereignty and intervention, and it lays out a number of intriguing questions for future research. There is no convincing statement that a decisive break has been made along the continuum from state sovereignty to legitimate international community intervention. What is left to be done is the very difficult work of defining what is a legitimate intervention. On what issues and under what conditions there exists enough consensus of values and interests among the society of states to constitute an international community is another important question. It also remains to be seen at what point a decisive break from the old sovereignty concepts will be made with regard to the fundamental security issues of states. When that happens, a decisive step without return will have been made on the continuum.

Ecological Resistance Movements: The Global Emergence of Radical and Popular Environmentalism. Edited by Bron Raymond Taylor. Albany: State University of New York Press, 1995. 422p. $19.95 paper.

Robert Paehlke, *Trent University*

This book offers a series of articles about environmental organizations that are characterized as "radical and popular" across virtually the whole of the planet. How organizations qualify as radical and popular is not defined with precision, but only in the negative—radical and popular is not "well-established and mainstream." There is a very useful emphasis on environmentalism in poorer nations. Indeed, the combination of locales, organizations, and events covered in this volume makes this a useful addition to the literature on contemporary social movements.

Part I contains four articles on "popular ecological resistance" in the Americas, Part II four articles on Asia and the Pacific, Part III two articles on Africa, and Part IV three articles on Europe. Part V offers five articles that reflect more broadly on "the global emergence of popular ecological resistance," though one of these articles is wholly about Greenpeace, an organization not easily categorized as to locale. Within the North American context the articles are about Earth First! and the environmental justice movement. The Asia and the Pacific section has an article that looks at resistance to resource development within native communities in Asia, Central America, and Quebec. This latter combination of groups is in fact linked within an international umbrella organization called the Indigenous Environmental Network.

There is to my knowledge no single volume that reviews so comprehensively and so globally the range of emerging and radical environmental organizations. Previously, scholars of the environmental movement and environmentalism have emphasized the conservation movement, especially its nineteenth-century origins, and the modern environmental movement of the 1960s and 1970s. There are now a number of excellent books on the history of the Sierra Club, the ideas of Aldo Leopold, and the transitions associated with Earth Day. In more recent years, however, the environmental movement has indeed broadened and spread, and some of the newer organizations, both here and elsewhere, are very different in character and focus from the now more established and, some would say, cautious environmental organizations.

This distinction is a matter of degree and perspective, of course. From the perspective of the (anti-environmentalist) wise use movement and the opposition to takings, the Sierra Club, Greenpeace, and Earth First! are of a kind. But elsewhere in the world, as this volume makes clear, the takings come very much from the other direction—some multinational corporations do act very differently in the varying global contexts within which they operate. Elsewhere in the world takings are not environmentalist-advanced legislative impositions on property rights (by some definitions of those rights) but physical denials of sustainable ecosystems and human livelihoods by externally advanced resource extraction. The authors in this volume identify and clearly sympathize with the wide variety of groups who resist these latter impositions.

Some will find the analysis insufficiently detached and objective, especially in the sometimes-conveyed assumption that these newer, more resistance-oriented groups are part of a rising tide of resistance within the poorer nations. The assessments are, perhaps, a touch romantic at times, but most of the time they are not. For example, the resisting villagers of Thailand are very clearly distinguished in perspective from advocates of wildlife conservation or deep ecology. Another interesting dimension of the analysis is the common pattern that finds the newer ecological groups defending local governance and autonomy against national or international authority. This is a theme not unknown among conservatives in the United States who would resist most environmental protection initiatives.

Nor is the importance of national and international environmental protection lost on the authors of the articles in this volume, despite their doubts about established environmentalism. As Taylor himself puts it, "While radical environmentalists nearly always . . . desire comprehensive decentralization, many know that the most important environmental victories have resulted from federal legislation or legal rulings which contravene the desires of local communities. This has been especially true in the United States, as well as in Australia, where large tracts of Tasmanian forest were saved by federal power, not public sentiment" (p. 341).

What this volume offers that is distinctive is an intelligent and truly global assessment of environmental actions and organizations that are not readily found. They are rarely located in the phone books of large metropolitan areas or even in organizational directories. Some are highly ephemeral, but all are very much a part of the larger process of globalization and industrial development within which a higher ecological price is frequently paid in locales lacking developed environmental rules, agencies, organizations, and history. Such organizations can (and do) become complacent, but their absence can all but guarantee the often tragic narratives that this volume conveys, and their presence is not so secure as it is sometimes easy to assume.

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Palmer, Michael J.
Palmquist, Bradley
Pampel, Fred
Pangle, Thomas
Parel, Anthony
Parish, William
Patterson, Dennis
Patterson, Thomas
Peng, Yali
Penubarti, Mohan
Percy, Stephen L.
Perry, H. W., Jr.
Peters, B. Guy
Peterson, Paul
Petrocik, John
Phillips, Anne
Piazza, Thomas
Pinderhughes, Dianne M.
Pinkard, Terry
Pippin, Robert B.
Pitkin, Hanna
Piven, Frances Fox
Platt, Michael
Plutzer, Eric
Pogge, Thomas
Pollins, Brian M.
Polsby, Nelson
Pool, Jonathan
Poole, Keith
Porter, David
Posner, Richard
Potters, Jan
Powell, G. Bingham, Jr.
Powell, Lynda W.
Powell, Robert
Price, Vincent
Provine, Doris Marie
Przeworski, Adam
Puchala, Donald
Putnam, Robert D.

Quiñones, Sherry Bennett

Rabinowitz, George
Radcliff, Benjamin
Rae, Douglas
Ragin, Charles C.
Ragsdale, Lyn
Rahe, Paul A.
Rahn, Wendy
Rainey, Hal
Ranney, J. Austin
Rasch, Bjorn-Erik
Rasmussen, Eric B.
Rawski, Thomas
Ray, James Lee
Raymond, Gregory A.
Reed, Adolph L., Jr.
Regan, Patrick
Regens, James
Reich, Simon
Reisinger, William
Rice, Tom
Rich, Michael J.
Richardson, Bradley
Riedinger, Jeffrey
Rietz, Thomas A.
Riezman, Raymond G.
Riggle, Ellen
Riley, Jonathan

Riley, Patrick
Ring, Jennifer
Ringquist, Evan
Rivers, Douglas
Roberts, Brian
Robertson, John
Robinson, Donald
Rockman, Bert
Roeder, Philip
Roelofs, H. Mark
Rogers, James M.
Rogin, Michael
Rogowski, Ronald
Rohde, David W.
Rohr, John A.
Rohrschneider, Robert
Rohter, Ira
Romzek, Barbara
Rosano, Michael
Rose, Richard
Rose-Ackerman, Susan
Rosen, Stanley
Rosenbloom, David
Rosenblum, Nancy
Rosenbluth, Frances
Rosenthal, Howard
Rothchild, Donald S.
Rothenberg, Lawrence
Rubenstein, Diane
Ruderman, Richard
Rule, James B.
Russett, Bruce
Rutten, Andrew
Ryan, Alan

Saari, Donald
Sabatier, Paul
Salisbury, Robert
Salkever, Stephen G.
Saltzstein, Grace Hall
Sanders, David
Sanders, Lynn
Sanderson, Steven
Sargent, Thomas J.
Scaff, Lawrence
Schaefer, David
Schick, Allen
Schlesinger, Joseph A.
Schlozman, Kay Lehman
Schneider, Mark
Schneider, Saundra
Schofield, Norman
Scholz, John T.
Schrodt, Philip A.
Schwartz, Edward
Schwartz, Nancy
Schwartz, Thomas
Schwebach, Valerie
Scott, John T.
Scully, Timothy
Sears, David
Segal, Jeffrey
Segal, Jennifer
Seligson, Mitchell
Shamir, Michal
Shapiro, Ian
Shapiro, Martin
Shapiro, Michael
Shapiro, Richard
Shapiro, Robert
Sharkansky, Ira
Shaw, Daron
Sheehan, Reginald
Shefter, Martin
Shell, Susan M.
Shepsle, Kenneth
Shi, Tianjian
Shiffman, Gary
Shipan, Charles
Shue, Vivienne
Shugart, Matthew
Shvetsova, Olga
Sica, Alan
Siegelman, Carol
Sigel, Roberta
Sigelman, Lee
Silver, Brian D.
Simmons, Beth
Simon, Adam
Simonton, Dean
Simowitz, Roslyn
Sinclair, Barbara
Sinopoli, Richard
Siverson, Randolph
Skillen, James W.
Skowronek, Stephen
Slivinski, Al
Smiley, Marion
Smith, Alastair
Smith, Anna Marie
Smith, Charles
Smith, Renée M.
Smith, Rogers M.
Smith, Tom
Smithey, Shannon K.
Sniderman, Paul
Snyder, Jack L.
Snyder, James
Sobel, Joel
Soltan, Karol
Songer, Donald
Sorauf, Frank
Spaeth, Harold
Spinner, Jeff
Spiro, David E.
Spragens, Thomas A., Jr.
Sprague, John
Spriggs, James F., II
Squire, Peverill
Stam, Allan C., III
Starr, Harvey
Stein, Barry
Stein, Janice
Stein, Robert
Steinberger, Peter
Steinmo, Sven
Stephens, Evelyne Huber
Stephens, John
Sterba, James
Stevenson, Randolph
Stewart, Charles
Stewart, Joseph, Jr.
Stewart, Marianne C.
Stewart, Philip
Stiehm, Judith
Stillman, Peter G.
Stimson, James
Stimson, Shannon
Stockton, Ronald
Stokes, Donald E.
Stokes, Susan
Stoll, Richard
Stone, Clarence
Stone, Deborah
Stone, Walter J.
Stoner-Weiss, Kathryn
Strate, John M.
Strumpf, Koleman S.
Sugden, Robert
Sullivan, Terry
Sullivan, Vickie B.
Swank, Duane H.
Swanson, Judith A.
Swedberg, Richard
Swistak, Piotr

Taagepera, Rein
Taber, Charles S.
Tarcov, Nathan
Tarrow, Sidney
Tate, C. Neal
Tate, Katherine
Taylor, Charles
Tedin, Kent L.
Teske, Nathan
Teske, Paul
Tessitore, Aristide
Tetlock, Philip E.
Theiss-Morse, Elizabeth
Thiele, Leslie Paul
Thomas, Sue
Thompson, Norma
Thompson, William R.
Timpone, Richard J.
Tinder, Glenn
Tong, James
Tsebelis, George

Uhlaner, Carole J.
Uslaner, Eric M.

van de Walle, Nicolas
Van Dyke, Vernon
Varshney, Ashutosh
Vasquez, John A.
Velkley, Richard

Ventura, Jaume
Verba, Sidney
Viotti, Paul Richard
Vogel, David
Vogel, Steven

Wagner, R. Harrison
Wahlbeck, Paul J.
Walker, James M.
Walker, Thomas G.
Wallerstein, Michael
Waltz, Kenneth N.
Wang, T.Y.
Wantchekon, Leonard
Ward, James
Warren, Mark E.
Warwick, Paul V.
Waterman, Richard W.
Wattenberg, Martin P.
Weatherford, M. Stephen
Weber, Paul J.
Weber, Ronald E.
Weil, Frederick D.
Weil, Laurence A.
Weimer, David L.
Weinberger, Jerry
Weingast, Barry R.
Weisberg, Herbert F.
Weissert, Carol S.
Weithman, Paul J.
Wellhofer, E. Spencer
Wendt, Alexander
Werner, Suzanne
West, Darrell M.
Western, Bruce
Whitby, Kenny J.
White, Stephen K.
Wilcox, Clyde
Williams, John T.
Williams, Kenneth C.
Wilson, John Douglas
Wilson, Rick K.
Winthrop, Delba
Witte, John F.
Wittkopf, Eugene R.
Wittman, Donald
Wlezien, Christopher
Wolfinger, Raymond E.
Wolin, Richard
Wolin, Sheldon S.
Wood, Allen W.
Wood, B. Dan
Wood, Gordon S.
Woolley, John
Wright, Gerald C.
Wright, John R.

Xenos, Nicholas

Yack, Bernard
Yarbrough, Beth
Yilmaz, Mustafa R.
Young, Iris Marion
Young, M. Crawford
Young, Oran R.

Zagare, Frank C.
Zarefsky, David
Zeckhauser, Richard J.
Zeng, Langche
Zerilli, Linda
Zuckerman, Alan S.
Zuckert, Catherine H.
Zuckert, Michael P.

INDEX TO VOLUME 90

Abramson, Paul R., and Ronald Inglehart. *Value Change in Global Perspective.* Reviewed by Raymond M. Duch 665
Achen, Christopher H., and W. Phillips Shively. *Cross-Level Inference.* Reviewed by Henry E. Brady 399
Acosta-Bélen, Edna, see Bose, Christine E. 690
Agüero, Felipe. *Soldiers, Civilians, and Democracy: Post-Franco Spain in Comparative Perspective.* Reviewed by Donald Share 928
Ahrensdorf, Peter J. *The Death of Socrates and the Life of Philosophy.* Reviewed by Mark J. Lutz 638
Aimer, Peter, see Vowles, Jack 948
Alexander, Herbert E., and Anthony Corrado. *Financing the 1992 Election.* Reviewed by Frank J. Sorauf 417
Alexander, Herbert E., and Rei Shiratori, eds. *Comparative Political Finance Among the Democracies.* Reviewed by Arthur A. Gunlicks 208
Alford, C. Fred. *Group Psychology and Political Theory.* Reviewed by James M. Glass 167
Allen, David S., and Robert Jensen, eds. *Freeing the First Amendment: Critical Perspectives on Freedom of Expression.* Reviewed by Donald A. Downs 904
Andrain, Charles F., and David E. Apter. *Political Protest and Social Change: Analyzing Politics.* Reviewed by Sidney Tarrow 874
Appleby, R. Scott, see Marty, Martin E. 678
Apter, David E., See Andrain, Chris F. 874
Archer, Robin. *Economic Democracy: The Politics of Feasible Socialism.* Reviewed by Peter Bachrach 400
Armor, David. *Forced Justice: School Desegregation and the Law.* Reviewed by Michael Strine 183
Aronson, Ronald. *After Marxism.* Reviewed by Brian Fay 168
Austen-Smith, David, and Jeffrey S. Banks. "Information Aggregation, Rationality, and the Condorcet Jury Theorem." 34
Badran, Margot. *Feminists, Islam, and Nation: Gender and the Making of Modern Egypt.* Reviewed by Masoud Kazemzadeh 929
Banks, Jeffrey S., see Austen-Smith, David 34
Banning, Lance. *The Sacred Fire of Liberty: James Madison and the Founding of the Federal Republic.* Reviewed by Vincent McGuire 884
Bar-Joseph, Uri. *Intelligence Intervention in the Politics of Democratic States: The United States, Israel, and Britain.* Reviewed by James J. Wirtz 230
Barany, Zoltan, see Volgyes, Ivan 687
Barber, Kathleen L. *Proportional Representation and Election Reform in Ohio.* Reviewed by V. Jerone Stephens 643
Barnes, Barry. *The Elements of Social Theory.* Reviewed by Daniel Little 619
Baron, David P. "A Dynamic Theory of Collective Goods Programs." 316
Barry, Brian. *Justice as Impartiality.* Reviewed by Charles Larmore 884
Bell, D. S., and Byron Criddle. *The French Communist Party in the Fifth Republic.* Reviewed by George Ross 209
Bell, Daniel A., David Brown, Kanishka Jayasuriya, and David M. Jones. *Towards Illiberal Democracy in Pacific Asia.* Reviewed by Ross Marlay 666
Benner, Erica. *Really Existing Nationalisms: A Post-Communist View from Marx and Engels.* Reviewed by David Miller 885
Bennett, D. Scott, and Allan C. Stam III. "The Duration of Interstate Wars, 1816–1985." 239
Bennett, Vivienne. *The Politics of Water: Urban Protest, Gender, and Power in Monterrey, Mexico.* Reviewed by Helen Ingram 931
Berger, Mark T. *Under Northern Eyes: Latin American Studies and U.S. Hegemony in the Americas, 1898–1990.* Reviewed by Eldon Kenworthy 952
Berkowitz, Peter. *Nietzsche: The Ethics of an Immoralist.* Reviewed by Bruce Detwiler 620
Bernauer, Thomas, see Schneider, Gerald 705
Best, Steven. *The Politics of Historical Vision: Marx, Foucault, Habermas.* Reviewed by Stephen K. White 621
Betz, Hans-Georg. *Radical Right-Wing Populism in Western Europe.* Reviewed by Peter H. Merkl 210
Bickers, Kenneth N., see Stein, Robert M. 661
Binder, Sarah A. "The Partisan Basis of Procedural Choice: Allocating Parliamentary Rights in the House, 1789–1990." 8
Birnbaum, Pierre, and Ira Katznelson. *Paths of Emancipation.* Reviewed by Benjamin Ginsberg 211
Black, Michael, see Fischer, Frank 695
Blank, Robert, and Janna C. Merrick. *Human Reproduction, Emerging Technologies and Conflicting Rights.* Reviewed by Cynthia R. Daniels 184
Boesche, Roger. *Theories of Tyranny from Plato to Arendt.* Reviewed by Larry Arnhart 887
Booth, William James. *Households: On the Moral Architecture of the Economy.* Reviewed by Scott Meikle 401
Borisov, Vadim, see Clarke, Simon 668
Borón, Atilio A. *State, Capitalism, and Democracy in Latin America.* Reviewed by Ronald H. Chilcote 932
Bose, Christine E., and Edna Acosta-Bélen, eds. *Women in the Latin American Development Process.* Reviewed by Rita Jalali 690
Bowman, Ann O'M., see Pagano, Michael A. 919
Box-Steffensmeier, Janet M., and Renée M. Smith. "The Dynamics of Aggregate Partisanship." 567
Bradley, Gerard V., see Grasso, Kenneth L. 627
Brams, Steven J. *Theory of Moves.* Reviewed by Michael B. Nicholson 169
Brennan, James P. *The Labor Wars in Córdoba, 1955–1976: Ideology, Work, and Labor Politics in an Argentine Industrial City.* Reviewed by Peter Ranis 446
Brennan, Mary C. *Turning Right in the Sixties: The Conservative Capture of the GOP.* Reviewed by William F. Connelly, Jr. 643
Brenner, Saul, and Harold J. Spaeth. *Stare Indecisis: The Alteration of Precedent on the Supreme Court, 1946–1992.* Reviewed by S. Sidney Ulmer 418
Brilmayer, Lea. *American Hegemony: Political Morality in a One-Superpower World.* Reviewed by Bruce Russett 231
Brown, David, see Bell, Daniel A. 666
Browne, William P. *Cultivating Congress: Constituents, Issues, and Interests in Agricultural Policymaking.* Reviewed by Scott H. Ainsworth 185
Brynen, Rex, Bahgat Korany, and Paul Noble, eds. *Political Liberalization and Democratization in the Arab World, volume 1: Theoretical Perspectives.* Reviewed by Augustus Richard Norton 933
Bubeck, Diemut. *Care, Gender, and Justice.* Reviewed by Wendy Gunther-Canada 888
Burke, Martin J. *The Conundrum of Class: Public Discourse on the Social Order in America.* Reviewed by Ira Katznelson 419
Burman, Stephen. *The Black Progress Question: Explaining the African American Predicament.* Reviewed by Paula D. McClain 867
Butler, David, and Martin Westlake. *British Politics and the European Elections 1994.* Reviewed by Michael Keating 447
Callinicos, Alex. *Theories and Narratives: Reflections on the Philosophy of History.* Reviewed by Joshua Foa Dienstag 402
Cameron, Charles, David Epstein, and Sharyn O'Halloran. "Do Majority-Minority Districts Maximize Substantive Black Representation in Congress?" 794
Campbell, Ballard C. *The Growth of American Government: Governance from the Cleveland Era to the Present.* Reviewed by Charles T. Goodsell 644
Campbell, Colin, and Bert A. Rockman, eds. *The Clinton Presidency: First Appraisals.* Reviewed by Norman C. Thomas 645
Carlton-Carew, Miranda A., see Snider, Don M. 706
Carnevale, David G. *Trustworthy Government: Leadership and Management Strategies for Building Trust and High Performance.* Reviewed by Frank J. Thompson 420
Carnoy, Martin. *Faded Dreams: The Politics and Economics of Race in America.* Reviewed by Paula D. McClain 867
Carpenter, Daniel P. "Adaptive Signal Processing, Hierarchy, and Budgetary Control in Federal Regulation." 283
Casper, Gretchen. *Fragile Democracies: The Legacies of Authoritarian Rule.* Reviewed by Belinda A. Aquino 667

Centeno, Miguel Angel. *Democracy Within Reason: Technocratic Revolution in Mexico.* Reviewed by Judith Gentleman 212

Cimbala, Stephen J. *Collective Insecurity: U.S. Defense Policy and the New World Disorder.* Reviewed by William C. Martel 691

Cimbala, Stephen J., ed. *Clinton and Post–Cold War Defense.* Reviewed by Robert J. Lieber 953

Claggett, William J. M., see Shafer, Byron E. 438

Clark, Terry Nichols, ed. *Urban Innovation: Creative Strategies for Turbulent Times.* Reviewed by Robert M. Stein 420

Clarke, Simon, Peter Fairbrother, and Vadim Borisov. *The Workers' Movement in Russia.* Reviewed by Darrell Slider 668

Clayton, Cornell W., ed. *Government Lawyers: The Federal Legal Bureaucracy and Presidential Politics.* Reviewed by Herbert M. Kritzer 422

Close, David. ed. *Legislatures and the New Democracies in Latin America.* Reviewed by Scott Mainwaring 213

Clucas, Richard A. *The Speaker's Electoral Connection: Willie Brown and the California Assembly.* Reviewed by David Soherr-Hadwiger 423

Coles, Romand. "Liberty, Equality, Receptive Generosity: Neo-Nietzschean Reflections on the Ethics and Politics of Coalition." 375

Connelly, William F., Jr., and John J. Pitney, Jr. *Congress' Permanent Minority? Republicans in the U.S. House.* Reviewed by Christopher J. Bailey 186

Conway, David. *Classical Liberalism: The Unvanquished Ideal.* Reviewed by James M. Glass 888

Cooper, Davina. *Power in Struggle: Feminism, Sexuality, and the State.* Reviewed by Susan Bickford 622

Cooper, Phillip J. *Battles on the Bench: Conflict Inside the Supreme Court.* Reviewed by Lawrence Baum 905

Cornford, Daniel, ed. *Working People of California.* Reviewed by Meta Mendel-Reyes 424

Corrado, Anthony, see Alexander, Herbert E. 417

Crewe, Ivor, and Anthony King. *SDP: The Birth, Life, and Death of the Social Democratic Party.* Reviewed by David E. Butler 934

Criddle, Byron, see Bell, D. S. 209

Cropsey, Joseph. *Plato's World: Man's Place in the Cosmos.* Reviewed by Christopher Bruell 623

Curry, Jane Lefwich, and Luba Fajfer, eds. *Poland's Permanent Revolution: People vs. Elites, 1956–1990.* Reviewed by Robert Zuzowski 935

Danforth, Loring M. *The Macedonian Conflict: Ethnic Nationalism in a Transitional World.* Reviewed by Adamantia Pollis 953

Davey, Joseph Dillon. *The New Social Contract: America's Journey from Welfare State to Police State.* Reviewed by Christopher Howard 426

Davidson, Chandler, and Bernard Grofman, eds. *Quiet Revolution in the South: The Impact of the Voting Rights Act 1965–1990.* Reviewed by Mark E. Rush 187

Davidson, Roger H., see Thurber, James A. 925

Dawisha, Adeed, and Karen Dawisha, eds. *The Making of Foreign Policy in Russia and the New States of Eurasia.* Reviewed by Glenn Chafetz 692

Dawisha, Karen, see Dawisha, Adeed 692

de la Garza, Rodolfo O., and Louis DeSipio, eds. *Ethnic Ironies: Latino Politics in the 1992 Elections.* Reviewed by Roberto E. Villarreal 906

DeCew, Judith Wagner, see Shapiro, Ian 413

DeLuca, Tom. *The Two Faces of Political Apathy.* Reviewed by Philip Green 890

DeSipio, Louis, see de la Garza, Rodolfo O. 906

Diamandouros, P. Nikiforos, see Gunther, Richard 672

Diamond, Larry, and Marc F. Plattner, eds. *Economic Reform and Democracy.* Reviewed by Anne E. Henderson 448

Dienstag, Joshua Foa. "Serving God and Mammon: The Lockean Sympathy in Early American Political Thought." 497

Digeser, Peter. *Our Politics, Our Selves? Liberalism, Identity, and Harm.* Reviewed by Ronald Beiner 171

Dobbs, Darrell. "Family Matters: Aristotle's Appreciation of Women and the Plural Structure of Society." 74

Domhoff, G. William, see Webber, Michael J. 824

Downs, George W., and David M. Rocke. *Optimal Imperfection? Domestic Uncertainty and Institutions in International Relations.* Reviewed by T. Clifton Morgan 693

Dryzek, John S. "Political Inclusion and the Dynamics of Democratization." 475

du Toit, Pierre. *State Building and Democracy in Southern Africa: Botswana, Zimbabwe, and South Africa.* Reviewed by James R. Scarritt 936

Duyvendak, Jan Willem, see Kriesi, Hanspeter 874

Easton, David, John G. Gunnell, and Michael B. Stein. *Regime and Discipline: Democracy and the Development of Political Science.* Reviewed by James W. Ceaser 171

Eisenach, Eldon J. *The Lost Promise of Progressivism.* Reviewed by R. Jeffrey Lustig 187

Eisenberg, Avigail I. *Reconstructing Political Pluralism.* Reviewed by Dennis Bathory 624

Eldersveld, Samuel J. *Party Conflict and Community Development: Postwar Politics in Ann Arbor.* Reviewed by William Crotty 188

Emberley, Peter C. *Values Education and Technology: The Ideology of Dispossession.* Reviewed by Elisabeth A. Kelly 173

Epp, Charles R. "Do Bills of Rights Matter? The Canadian Charter of Rights and Freedoms." 765

Epstein, David, see Cameron, Charles 794

Epstein, Lee, Jeffrey A. Segal, and Timothy Johnson. "The Claim of Issue Creation on the U.S. Supreme Court." 845

Esman, Milton J., and Shibley Telhami, eds. *International Organizations and Ethnic Conflict.* Reviewed by Manus I. Midlarsky 694

Evans, Peter. *Embedded Autonomy: States and Industrial Transformation.* Reviewed by Atul Kohli 669

Fairbrother, Peter, see Clarke, Simon 668

Fajfer, Luba, see Curry, Jane Lefwich 935

Farnen, Russell F. *Nationalism, Ethnicity, and Identity: Cross-National and Comparative Perspectives.* Reviewed by William Safran 670

Feagin, Joe R., see Smith, Michael Peter 867

Fearon, James D., and David D. Laitin. "Explaining Interethnic Cooperation." 715

Felstiner, William L. F., see Sarat, Austin 201

Ferry, Luc. *The New Ecological Order.* Reviewed by Joseph R. DesJardins 403

Filtzer, Donald. *Soviet Workers and the Collapse of Perestroika: The Soviet Labour Process and Gorbachev's Reforms, 1985–1991.* Reviewed by Darrell Slider 668

Finegold, Kenneth. *Experts and Politicians: Reform Challenges to Machine Politics in New York, Cleveland, and Chicago.* Reviewed by Clarence N. Stone 646

Fineman, Martha Albertson, and Isabel Karpin, eds. *Mothers in Law: Feminist Theory and the Legal Regulation of Motherhood.* Reviewed by Sue Davis 426

Finifter, Ada W. "Attitudes Toward Individual Responsibility and Political Reform in the Former Soviet Union." 138

Fischel, William A. *Regulatory Takings: Law, Economics, and Politics.* Reviewed by David A. Schultz 189

Fischer, Frank, and Michael Black, eds. *Greening Environmental Policy: The Politics of a Sustainable Future.* Reviewed by Karen T. Litfin 695

Fishkin, James S. *The Voice of the People: Public Opinion and Democracy.* Reviewed by Robert Y. Shapiro 625

Fitzgibbons, Athol. *Adam Smith's System of Liberty, Wealth, and Virtue: The Moral and Political Foundations of The Wealth of Nations.* Reviewed by Roger J. Fechner 626

Fowler, Robert Booth. *The Greening of Protestant Thought.* Reviewed by John F. Haught 404

Friedman, Barry D. *Regulation in the Reagan-Bush Era: The Eruption of Presidential Influence.* Reviewed by Richard W. Waterman 647

Friedman, Edward. *National Identity and Democratic Prospects in Socialist China.* Reviewed by Michael Dillon 214

Fulbrook, Mary. *Anatomy of a Dictatorship: Inside the GDR, 1949–1989.* Reviewed by Helga A. Welsh 936

Gadzey, Anthony Tuo-Kofi. *The Political Economy of Power: Hegemony and Economic Liberalism.* Reviewed by Kathleen R. McNamara 232

Gangi, William. *Saving the Constitution from the Courts.* Reviewed by Gary L. McDowell 648
Gedicks, Frederick Mark. *The Rhetoric of Church and State.* Reviewed by Stephen V. Monsma 907
Gelpi, Christopher, see Rousseau, David L. 512
Gerber, Scott Douglas. *To Secure These Rights: The Declaration of Independence and Constitutional Interpretation.* Reviewed by Mark A. Graber 190
Gerhardt, Michael J. *The Federal Impeachment Process: A Constitutional and Historical Analysis.* Reviewed by Mark J. Rozell 908
Gershoni, Israel, and James P. Jankowski. *Redefining the Egyptian Nation, 1930–1945.* Reviewed by Timothy P. Mitchell 450
Giddens, Anthony. *Beyond Left and Right: The Future of Radical Politics.* Reviewed by Timothy Fuller 174
Giddens, Anthony. *Politics, Sociology, and Social Theory: Encounters with Classical and Contemporary Social Thought.* Reviewed by Daniel Little 619
Gilbert, Mark. *The Italian Revolution: The End of Politics Italian Style?* Reviewed by Leonard Weinberg 670
Gilens, Martin. "'Race Coding' and White Opposition to Welfare." 593
Gill, Graeme. *The Collapse of a Single-Party System: The Disintegration of the Communist Party of the Soviet Union.* Reviewed by John P. Willerton 215
Gilliam, David, see Ingram, Helen 452
Gilmartin, Christina Kelley. *Engendering the Chinese Revolution: Radical Women, Communist Politics, and Mass Movements in the 1920s.* Reviewed by Jean C. Robinson 937
Gilmour, John B. *Strategic Disagreement: Stalemate in American Politics.* Reviewed by Sean Q. Kelly 909
Giugni, Marco G., see Kriesi, Hanspeter 874
Glasser, Theodore L., and Charles T. Salmon, eds. *Public Opinion and the Communication of Consent.* Reviewed by George E. Marcus 427
Goertz, Gary. *Contexts of International Politics.* Reviewed by Harvey Starr 232
Goodin, Robert E. "Institutionalizing the Public Interest: The Defense of Deadlock and Beyond." 331
Gormley, William T., Jr. *Everybody's Children: Child Care as a Public Problem.* Reviewed by Jane Waldfogel 649
Gran, Thorvald. *The State in the Modernization Process: The Case of Norway 1850–1970.* Reviewed by Christine Ingebritsen 216
Grant, Wyn. *Autos, Smog, and Pollution Control: The Politics of Air Quality Management in California.* Reviewed by John J. Kirlin 910
Grasso, Kenneth L., Gerard V. Bradley, and Robert P. Hunt, eds. *Catholicism, Liberalism, and Communitarianism: The Catholic Intellectual Tradition and the Moral Foundations of Democracy.* Reviewed by Simone Chambers 627
Grofman, Bernard, see Davidson, Chandler 187
Groseclose, Tim, and James M. Snyder, Jr. "Buying Supermajorities." 303
Gunnell, John G., see Easton, David 171
Gunther, Richard, P. Nikiforos Diamandouros, and Hans-Jürgen Puhle, eds. *The Politics of Democratic Consolidation: Southern Europe in Comparative Perspective.* Reviewed by Enrique A. Baloyra 672
Hacker, Kenneth L., ed. *Candidate Images in Presidential Elections.* Reviewed by Mary E. Stuckey 650
Haggard, Stephan, and Robert R. Kaufman. *The Political Economy of Democratic Transitions.* Reviewed by Anne E. Henderson 448
Hall, Melvin F. *Poor People's Social Movement Organizations: The Goal Is to Win.* Reviewed by Douglas R. Imig 428
Hampson, Fen Osler, with Michael Hart. *Multilateral Negotiations: Lessons from Arms Control, Trade and the Environment.* Reviewed by I. William Zartman 466
Handberg, Roger. *The Future of the Space Industry: Private Enterprise and Public Policy.* Reviewed by Nathan C. Goldman 651
Hanson, Stephen E., ed. *Can Europe Work? Germany and the Reconstruction of Postcommunist Societies.* Reviewed by Daniel L. Nelson 954
Hariman, Robert. *Political Style: The Artistry of Power.* Reviewed by John S. Nelson 405
Harris, Nigel. *The New Untouchables: Immigration and the New World Worker.* Reviewed by Anthony M. Messina 697
Hart, Michael, see Hampson, Fen Osler 466
Hekman, Susan J. *Moral Voices, Moral Selves: Carol Gilligan and Feminist Moral Theory.* Reviewed by Sara Weir 406
Helena, Catt, see Vowles, Jack 948
Herman, Didi. *Rights of Passage: Struggles for Lesbian and Gay Legal Equality.* Reviewed by Shane Phelan 938
Hesli, Vicki L., see Miller, Arthur H. 153
Hill, Kim Quaile. *Democracy in the Fifty States.* Reviewed by Gerald C. Wright 191
Himelfarb, Richard. *Catastrophic Politics: The Rise and Fall of the Medicare Catastrophic Coverage Act of 1988.* Reviewed by Christine L. Day 911
Hindess, Barry. *Discourses of Power: From Hobbes to Foucault.* Reviewed by Jacqueline Stevens 892
Hobson, John M., see Weiss, Linda 464
Hochschild, Jennifer L. *Facing Up to the American Dream: Race, Class, and the Soul of the Nation.* Reviewed by Joe R. Feagin 429
Hofrenning, Daniel J. B. *In Washington but Not of It: The Prophetic Politics of Religious Lobbyists.* Reviewed by Allen D. Hertzke 430
Honig, Bonnie, ed. *Feminist Interpretations of Hannah Arendt.* Reviewed by Jeffrey C. Isaac 628
Honneth, Axel. *The Struggle for Recognition: The Moral Grammar of Social Conflicts.* Reviewed by Horst Mewes 892
Hopkins, Jack W. *Policymaking for Conservation in Latin America: National Parks, Reserves, and the Environment.* Reviewed by Eduardo Silva 673
Horowitz, Asher, and Terry Maley, eds. *The Barbarism of Reason: Max Weber and the Twilight of Enlightenment.* Reviewed by David Sciulli 629
Hoston, Germaine A. *The State, Identity, and the National Question in China and Japan.* Reviewed by Suzanne Ogden 216
Hua, Shiping. *Scientism and Humanism: Two Cultures in Post-Mao China (1978–1989).* Reviewed by Lucian W. Pye 451
Huber, John D. "The Vote of Confidence in Parliamentary Democracies." 269
Huckfeldt, Robert, and John Sprague. *Citizens, Politics, and Social Communication: Information and Influence in an Election Campaign.* Reviewed by Alan S. Zuckerman 192
Hult, Karen M., see Walcott, Charles E. 441
Hunt, Robert P., see Grasso, Kenneth L. 627
Huth, Paul K., see Rousseau, David L. 512
Hyland, James L. *Democratic Theory: The Philosophical Foundations.* Reviewed by Ross Harrison 175
Imig, Douglas. *Poverty and Power: The Political Representation of Poor Americans.* Reviewed by Kay Lehman Schlozman 912
Inglehart, Ronald, see Abramson, Paul R. 665
Ingraham, Patricia Wallace. *The Foundation of Merit: Public Service in American Democracy.* Reviewed by Peri E. Arnold 652
Ingram, Helen, Nancy Laney, and David Gilliam. *Divided Waters: Bridging the U.S.-Mexico Border.* Reviewed by Stephen P. Mumme 452
Isaac, Jeffrey C. "A New Guarantee on Earth: Hannah Arendt on Human Dignity and the Politics of Human Rights." 61
Ivers, Gregg. *To Build A Wall: American Jews and the Separation of Church and State.* Reviewed by H. Frank Way 193
Iyob, Ruth. *The Eritrean Struggle for Independence: Domination, Resistance, Nationalism, 1941–1993.* Reviewed by Edmond J. Keller 698
Jackson, Donald W., see Riddlesperger, James W., Jr. 438
Jacobson, David. *Rights Across Borders: Immigration and the Decline of Citizenship.* Reviewed by Jeff Spinner-Halev 894
Jacoby, Sanford, ed. *The Workers of Nations: Industrial Relations in a Global Economy.* Reviewed by Jonas Pontusson 467
Jaffe, Sanford, see Vasquez, John A. 471
Jankowski, James P., see Gershoni, Israel 450
Jayasuriya, Kanishka, see Bell, Daniel A. 666
Jelen, Ted G., and Clyde Wilcox. *Public Attitudes toward Church and State.* Reviewed by J. Christopher Soper 913

Jelen, Ted G., ed. *Perspectives on the Politics of Abortion.* Reviewed by MaryAnne Borrelli ... 431
Jenkins, J. Craig, and Bert Klandermans, eds. *The Politics of Social Protest: Comparative Perspectives on States and Social Movements.* Reviewed by Sidney Tarrow ... 874
Jensen, Robert, see Allen, David S. ... 904
Johnson, James Turner, see Vasquez, John A. ... 471
Johnson, Timothy, see Epstein, Lee ... 845
Jones, Barry, and Michael Keating, eds. *The European Union and the Regions.* Reviewed by Jeffrey Anderson ... 452
Jones, Bryan D. *Reconceiving Decision-Making in Democratic Politics: Attention, Choice, and Public Policy.* Reviewed by Barry Rundquist ... 432
Jones, Bryan D., ed. *The New American Politics: Reflections on Political Change and the Clinton Administration.* Reviewed by Nicol C. Rae ... 194
Jones, David M., see Bell, Daniel A. ... 666
Jones, Peter. *Rights.* Reviewed by Jack Donnelly ... 631
Judge, David, Gerry Stoker, and Harold Wolman, eds. *Theories of Urban Politics.* Reviewed by Barbara Ferman ... 433
Junnosuke, Masumi. *Contemporary Politics in Japan.* Reviewed by T. J. Pempel ... 217
Kagel, John H., and Alvin E. Roth, eds. *The Handbook of Experimental Economics.* Reviewed by Kenneth C. Williams ... 632
Karpin, Isabel, see Fineman, Martha Albertson ... 426
Katz, Jonathan N., and Brian R. Sala. "Careerism, Committee Assignments, and the Electoral Connection." ... 21
Katzelson, Ira, see Birnbaum, Pierre ... 211
Kaufman, Robert R., see Haggard, Stephan ... 448
Kay, W. D. *Can Democracies Fly in Space? The Challenge of Revitalizing the U.S. Space Program.* Reviewed by Radford Byerly, Jr. ... 653
Keating , Michael, see Jones, Barry ... 452
Kendall, Kathleen E., ed. *Presidential Campaign Discourse: Strategic Communication Problems.* Reviewed by Darrell M. West ... 434
Kenworthy, Eldon. *America/Americas: Myth in the Making of US Policy Toward Latin America.* Reviewed by Martha Cottam ... 218
Keren, Michael. *Professionals against Populism: The Peres Government and Democracy.* Reviewed by Jerome Slater ... 674
Kessler, Daniel, and Keith Krehbiel. "Dynamics of Cosponsorship." ... 555
Kim, Hyung-Ki, Michio Muramatsu, and T. J. Pempel, eds. *The Japanese Civil Service and Economic Development: Catalysts of Change.* Reviewed by Robert C. Angel ... 939
King, Anthony, see Crewe, Ivor ... 934
King, Desmond. *Separate and Unequal: Black Americans and the US Federal Government.* Reviewed by Paula D. McClain ... 867
Klandermans, Bert, see Jenkins, J. Craig ... 874
Kley, Roland. *Hayek's Social and Political Thought.* Reviewed by Gordon Tullock ... 895
Kline, Stephen Jay. *Conceptual Foundations for Multidisciplinary Thinking.* Reviewed by Courtney Brown ... 407
Kluegel, James R., David S. Mason, and Bernard Wegener, eds. *Social Justice and Political Change: Public Opinion in Capitalist and Post-Capitalist States.* Reviewed by Euel Elliott ... 676
Klyza, Christopher McGrory. *Who Controls Public Lands?: Mining, Forestry and Grazing Policies, 1870–1990.* Reviewed by Robert H. Nelson ... 914
Kolstoe, Paul. *Russians in the Former Soviet Republics.* Reviewed by Rasma Karklins ... 699
Koopmans, Ruud, see Kriesi, Hanspeter ... 874
Koopmans, Ruud. *Democracy from Below: New Social Movements and the Political System in West Germany.* Reviewed by Russell J. Dalton ... 453
Korany, Bahgat, see Brynen, Rex ... 933
Kramer, Matthew H. *Critical Legal Theory and the Challenge of Feminism: A Philosophical Reconception.* Reviewed by Kristin Bumiller ... 175
Krehbiel, Keith, see Kessler, Daniel ... 555
Kriesi, Hanspeter, Ruud Koopmans, Jan Willem Duyvendak, and Marco G. Giugni, eds. *New Social Movements in Western Europe: A Comparative Analysis.* Reviewed by Sidney Tarrow ... 874
Kroll, John A. *Closure in International Politics: The Impact of Strategy, Blocs, and Empires.* Reviewed by Carolyn Rhodes ... 700
Kupchan, Charles A, ed. *Nationalism and Nationalities in the New Europe.* Reviewed by Donald Rothchild ... 454
Kymlicka, Will. *Multicultural Citizenship.* Reviewed by Charles Taylor ... 408
Laitin, David D., see Fearon, James D. ... 715
Lamare, Jim, see Vowles, Jack ... 948
Lampert, Laurence. *Leo Strauss and Nietzsche.* Reviewed by Edward Andrew ... 633
Landau, Jacob M. *Pan-Turkism: From Irredentism to Cooperation.* Reviewed by Frank Tachau ... 955
Landy, Marc K., and Martin Levin. *The New Politics of Public Policy.* Reviewed by Robert F. Rich ... 195
Laney, Nancy, see Ingram, Helen ... 452
Lavrakas, Paul J., Michael W. Traugott, and Peter V. Miller, eds. *Presidential Polls and the News Media.* Reviewed by S. Robert Lichter ... 654
Legro, Jeffrey W. "Culture and Preferences in the International Cooperation Two-Step." ... 118
Lehmann, Scott. *Privatizing Public Lands.* Reviewed by Elinor Ostrom ... 196
Levin, Martin A., see Landy, Marc K. ... 195
Lichbach, Mark Irving. *The Rebel's Dilemma.* Reviewed by Anthony J. Gill ... 455
Lijphart, Arend. "The Puzzle of Indian Democracy: A Consociational Interpretation." ... 258
Lipschutz, Ronnie D., ed. *On Security.* Reviewed by Nicholas Onuf ... 468
Locke, Richard M. *Remaking the Italian Economy.* Reviewed by Stephen Hellman ... 456
Lodge, Milton, and Kathleen McGraw, eds. *Political Judgement: Structure and Process.* Reviewed by James H. Kuklinski ... 654
Loewenhardt, John. *The Reincarnation of Russia: Struggling with the Legacy of Communism, 1990–1994.* Reviewed by Nicolai N. Petro ... 940
Lorber, Judith. *Paradoxes of Gender.* Reviewed by Terrell Carver ... 634
Luciak, Ilja A. *The Sandinista Legacy: Lessons from a Political Economy in Transition.* Reviewed by Gary Prevost ... 941
Lustick, Ian S. "History, Historiography, and Political Science: Multiple Historical Records and the Problem of Selection Bias." ... 605
Lyons, Gene M., and Michael Mastanduno, eds. *Beyond Westphalia? State Sovereignty and International Intervention.* Reviewed by Daniel J. Whiteneck ... 956
Machan, Tibor R., and Douglas B. Rasmussen, eds. *Liberty for the 21st Century: Contemporary Libertarian Thought.* Reviewed by James M. Glass ... 888
MacManus, Susan A., with Patricia A. Turner. *Young v. Old: Generational Combat in the 21st Century.* Reviewed by William G. Mayer ... 915
Maley, William, see Saikal, Amin ... 460
Maltzman, Forrest, and Paul Wahlbeck. "Strategic Policy Considerations and Voting Fluidity on the Burger Court." ... 581
Manion, Melanie. "The Electoral Connection in the Chinese Countryside." ... 736
Manzer, Robert A. "Hume's Constitutionalism and the Identity of Constitutional Democracy." ... 488
Marcus, George E., John L. Sullivan, Elizabeth Theiss-Morse, and Sandra L. Wood. *With Malice toward Some: How People Make Civil Liberties Judgments.* Reviewed by James L. Gibson ... 916
Marklew, Victoria. *Cash, Crisis, and Corporate Governance: The Role of National Financial Systems in Industrial Restructuring.* Reviewed by Nikolaos Zahariadis ... 677
Marshall, John. *John Locke: Resistance, Religion, and Responsibility.* Reviewed by Ian C. Harris ... 176
Marty, Martin E., and R. Scott Appleby, eds. *Fundamentalism Comprehended.* Reviewed by David C. Rapoport ... 678
Martz, John D., ed. *United States Policy in Latin America: A Decade of Crisis and Challenge.* Reviewed by William M. LeoGrande ... 469
Mason, David S., see Kluegel, James R. ... 676
Matheny, Albert R., see Williams, Bruce A. ... 445
Maxwell, Kenneth. *The Making of Portuguese Democracy.* Reviewed by Walter C. Opello, Jr. ... 457

May, Todd. *The Moral Theory of Poststructuralism.* Reviewed by Brent L. Pickett ... 409

Mayer, William G., ed. *In Pursuit of the White House: How We Choose Our Presidential Nominees.* Reviewed by Ryan J. Barilleaux ... 655

Maynard-Moody, Steven. *The Dilemma of the Fetus: Fetal Research, Medical Progress, and Moral Politics.* Reviewed by Patricia Boling ... 917

McCarthy, Patrick. *The Crisis of the Italian State: From the Origins of the Cold War to the Fall of Berlusconi.* Reviewed by Leonard Weinberg ... 670

McClain, Paula D. "Black Politics at the Crossroads? Or in the Cross-Hairs?" ... 867

McCoy, Jennifer, Andrés Serbin, William C. Smith, and Andrés Stambouli, eds. *Venezuelan Democracy under Stress.* Reviewed by Sylvia Maxfield ... 219

McCready, Amy R. "The Ethical Individual: An Historical Alternative to Contemporary Conceptions of the Self." ... 90

McGraw, Kathleen, see Lodge, Milton ... 654

McGuire, Kevin T., and Barbara Palmer. "Issues, Agendas, and Decision Making on the Supreme Court." ... 853

McNulty, John E., see Nagel, Jack H. ... 780

McWilliams, Wilson Carey. *The Politics of Disappointment: American Elections 1976–94.* Reviewed by Bryan D. Jones ... 196

Meier, Heinrich. *Carl Schmitt and Leo Strauss: The Hidden Dialogue.* Reviewed by Shadia B. Drury ... 410

Melzer, Arthur M. "The Origin of the Counter-Enlightenment: Rousseau and the New Religion of Sincerity." ... 344

Melzer, Arthur M., Jerry Weinberger, and M. Richard Zinman, eds. *History and the Idea of Progress.* Reviewed by George Klosko ... 177

Mershon, Carol A. "The Costs of Coalition: Coalition Theories and Italian Governments." ... 534

Middlebrook, Kevin J. *The Paradox of Revolution: Labor, the State, and Authoritarianism in Mexico.* Reviewed by Charles L. Davis ... 220

Miller, Arthur H., William M. Reisinger, and Vicki L. Hesli. "Understanding Political Change in Post-Soviet Societies: A Further Commentary on Finifter and Mickiewicz." ... 153

Miller, Benjamin. *When Opponents Cooperate: Great Power Conflict and Collaboration in World Politics.* Reviewed by Stephen R. Rock ... 233

Miller, David, and Michael Walzer, eds. *Pluralism, Justice, and Equality.* Reviewed by Avigail I. Eisenberg ... 635

Miller, David. *On Nationality.* Reviewed by Jeff Spinner-Halev ... 894

Miller, Marian A. L. *The Third World in Global Environmental Politics.* Reviewed by Amitrajeet A. Batabyal ... 701

Miller, Peter V., see Lavrakas, Paul J. ... 654

Miller, Raymond, see Vowles, Jack ... 948

Mitchell, Joshua. *The Fragility of Freedom: Tocqueville on Religion, Democracy, and the American Future.* Reviewed by Michael P. Zuckert ... 411

Moeser, John V., see Silver, Christopher ... 867

Moghadam, Valentine M., ed. *Democratic Reform and the Position of Women in Transitional Economies.* Reviewed by Barbara Jancar-Webster ... 679

Mondak, Jeffery J. *Nothing to Read: Newspapers and Elections in a Social Experiment.* Reviewed by Marion R. Just ... 918

Morris, Stephen D. *Political Reformism in Mexico: An Overview of Contemporary Mexican Politics.* Reviewed by Roderic Ai Camp ... 942

Moser, Peter. "The European Parliament as a Conditional Agenda Setter: What Are the Conditions? A Critique of Tsebelis (1994)." ... 834

Moyano, María José. *Argentina's Lost Patrol: Armed Struggle, 1969–1979.* Reviewed by Deborah L. Norden ... 458

Muramatsu, Michio, see Kim, Hyung-Ki ... 939

Nadon, Christopher. "From Republic to Empire: Political Revolution and the Common Good in Xenophon's *Education of Cyrus*." ... 361

Nagel, Jack H., and John E. McNulty. "Partisan Effects of Voter Turnout in Senatorial and Gubernatorial Elections." ... 780

Nataf, Daniel. *Democratization and Social Settlements: The Politics of Change in Contemporary Portugal.* Reviewed by Walter C. Opello, Jr. ... 457

Navarro, Armando. *Mexican American Youth Organization: Avant Garde of the Chicano Movement in Texas.* Reviewed by Benjamin Marquez ... 656

Nichol, James P. *Diplomacy in the Former Soviet Republics.* Reviewed by Glenn Chafetz ... 692

Nissen, Bruce. *Fighting For Jobs: Case Studies of Labor-Community Coalitions Confronting Plant Closings.* Reviewed by John Portz ... 197

Noble, Paul, see Brynen, Rex ... 933

Norden, Deborah L. *Military Rebellion in Argentina: Between Coups and Consolidation.* Reviewed by Gerardo L. Munck ... 943

Norman, Richard. *Ethics, Killing and War.* Reviewed by Robert L. Holmes ... 412

Nussbaum, Martha. *Poetic Justice: The Literary Imagination and Public Life.* Reviewed by Francis A. Beer ... 636

O'Halloran, Sharyn, see Cameron, Charles ... 794

O'Neill, John. *The Missing Child in Liberal Theory: Towards a Covenant Theory of: Family, Community, Welfare and the Civic State.* Reviewed by Shelley Burtt ... 178

O'Sullivan, Arthur, Terri A. Sexton, and Steven M. Sheffrin, eds. *Property Taxes and Tax Revolts: The Legacy of Proposition 13.* Reviewed by David Lowery ... 198

Orr, Susan. *Jerusalem and Athens: Reason and Revelation in the Works of Leo Strauss.* Reviewed by Michael Palmer ... 412

Orttung, Robert W. *From Leningrad to St. Petersburg: Democratization in a Russian City.* Reviewed by Jeffrey W. Hahn ... 459

Pagano, Michael A., and Ann O'M. Bowman. *Cityscapes and Capital: The Politics of Urban Development.* Reviewed by Richard E. DeLeon ... 919

Paidar, Parvin. *Women and the Political Process in Twentieth-Century Iran.* Reviewed by Masoud Kazemzadeh ... 929

Palmer, Barbara, see McGuire, Kevin T. ... 853

Palmer, Michael, and Thomas L. Pangle, eds. *Political Philosophy and the Human Soul: Essays in Honor of Allan Bloom.* Reviewed by Fred M. Frohock ... 637

Pangle, Thomas, see Palmer, Michael ... 637

Parens, Joshua. *Metaphysics as Rhetoric: Alfarabi's Summary of Plato's Laws.* Reviewed by Mark J. Lutz ... 638

Paris, David C. *Ideology and Educational Reform: Themes and Theories in Public Education.* Reviewed by Stephen G. Salkever ... 435

Patel, Kant, and Mark E. Rushefsky. *Health Care Politics and Policy in America.* Reviewed by Robert P. Rhodes ... 436

Paul, T. V. *Asymmetric Conflicts: War Initiation by Weaker Powers.* Reviewed by Ben D. Mor ... 234

Payne, Leigh A. *Brazilian Industrialists and Democratic Change.* Reviewed by Frances Hagopian ... 221

Payne, Richard J. *The Clash with Distant Cultures: Values, Interests and Force in American Foreign Policy.* Reviewed by David Campbell ... 702

Pemple, T. J., see Kim, Hyung-Ki ... 939

Perry, Elizabeth J. *Shanghai on Strike: The Politics of Chinese Labor.* Reviewed by Sidney Tarrow ... 874

Peterson, Paul E. *The Price of Federalism.* Reviewed by John E. Chubb ... 437

Petro, Nicolai N. *The Rebirth of Russian Democracy: An Interpretation of Political Culture.* Reviewed by Archie Brown ... 680

Pierce, Roy. *Choosing the Chief: Presidential Elections in France and the United States.* Reviewed by Michael Lewis-Beck ... 222

Pinello, Daniel R. *The Impact of Judicial-Selection Method on State-Supreme-Court Policy: Innovation, Reaction, and Atrophy.* Reviewed by Henry R. Glick ... 920

Pitney, John J., Jr., see Connelly, Jr., William F. ... 186

Plattner, Marc F., see Diamond, Larry ... 448

Pollins, Brian M. "Global Political Order, Economic Change and Armed Conflict: Coevolving Systems and the Use of Force." ... 103

Powell, Robert. "Uncertainty, Shifting Power, and Appeasement." ... 749

Press, Daniel. *Democratic Dilemmas in the Age of Ecology: Trees and Toxics in the American West.* Reviewed by Douglas J. Amy ... 199

Puhle, Hans-Jürgen, see Gunther, Richard ... 672

Ramsay, Meredith. *Community, Culture and Economic Development: The Social Roots of Local Action.* Reviewed by Gerry Stoker 920
Rasmussen, Douglas B., see Machan, Tibor R. 888
Ray, James Lee. *Democracy and International Conflict: An Evaluation of the Democratic Peace Proposition.* Reviewed by William J. Dixon 703
Redish, Martin H. *The Constitution as Political Structure.* Reviewed by William Gangi 200
Reisinger, William M., see Miller, Arthur H. 153
Reiter, Dan, see Rousseau, David L. 512
Reno, William. *Corruption and State Politics in Sierra Leone.* Reviewed by Barry Riddell 460
Rice, Condoleezza, see Zelikow, Philip 112
Richardson, Jeremy, see Whitely, Paul 227
Riddlesperger, Jr., James W., and Donald W. Jackson, eds. *Presidential Leadership and Civil Rights Policy.* Reviewed by Hugh Davis Graham 438
Rocke, David M., see Downs, George W. 693
Rockman, Bert A., see Campbell, Colin 645
Roe, Emery. *Narrative Policy Analysis: Theory and Practice.* Reviewed by Charles Lester 657
Roett, Riordan, ed. *The Challenge of Institutional Reform in Mexico.* Reviewed by Kevin J. Middlebrook 223
Rosen, Stanley. *Plato's* Statesman: *The Web of Politics.* Reviewed by Mark J. Lutz 638
Rosenfeld, Michel, ed. *Constitutionalism, Identity, Difference, and Legitimacy: Theoretical Perspectives.* Reviewed by John E. Finn 224
Ross, George. *Jacques Delors and European Integration.* Reviewed by Leon Hurwitz 704
Ross, Robert S. *Negotiating Cooperation: The United States and China, 1969–1989.* Reviewed by David Shambaugh 236
Ross, Robert S., ed. *East Asia in Transition: Toward a New Regional Order.* Reviewed by Donald Crone 470
Roth, Alvin E., see Kagel, John H. 632
Rousseau, David L., Christopher Gelpi, Dan Reiter, and Paul K. Huth. "Assessing the Dyadic Nature of the Democratic Peace, 1918–88." 512
Rowlands, Ian H. *The Politics of Global Atmospheric Change.* Reviewed by Karen T. Litfin 695
Ruderman, Anne Crippen. *The Pleasures of Virtue: Political Thought in the Novels of Jane Austen.* Reviewed by Ethan Fishman 896
Rushefsky, Mark E., see Patel, Kant 436
Saikal, Amin, and William Maley, eds. *Russia in Search of Its Future.* Reviewed by Stephen White 460
Sala, Brian R., see Katz, Jonathan N. 21
Salla, Michael E., see Summy, Ralph 471
Salmon, Charles T., see Glasser, Theodore L. 427
Sarat, Austin, and William L. F. Felstiner. *Divorce Lawyers and Their Clients: Power and Meaning in the Legal Process.* Reviewed by Roy B. Flemming 201
Scammell, Margaret. *Designer Politics: How Elections Are Won.* Reviewed by Anthony King 461
Schluchter, Wolfgang. *Paradoxes of Modernity: Culture and Conduct in the Theory of Max Weber.* Reviewed by Nancy L. Schwartz 896
Schmidt, David. *Rational Choice and Moral Agency.* Reviewed by Gerald F. Gaus 179
Schneider, Gerald, Patricia Weitsman, and Thomas Bernauer, eds. *Towards a New Europe: Stops and Starts in Regional Integration.* Reviewed by Robert Leonardi 705
Schneider, Saundra K. *Flirting with Disaster: Public Management in Crisis Situations.* Reviewed by Louise K. Comfort 658
Schultz, David A., and Christopher E. Smith. *The Jurisprudential Vision of Justice Antonin Scalia.* Reviewed by Richard A. Brisbin, Jr. 921
Schulz, Donald E., and Edward J. Williams, eds. *Mexico Faces the 21st Century.* Reviewed by Joseph L. Klesner 681
Schwartz, Joseph M. *The Permanence of the Political: A Democratic Critique of the Radical Impulse to Transcend Politics.* Reviewed by Benjamin Barber 640
Schwerin, Edward W. *Mediation, Citizen Empowerment, and Transformational Politics.* Reviewed by Richard J. Maiman 203
Sclove, Richard E. *Democracy and Technology.* Reviewed by Sheila Jasanoff 659
Scott, Catherine V. *Gender and Development: Rethinking Modernization and Dependency Theory.* Reviewed by Ann Marie Goetz 225
Seddon, David, see Walton, John 708
Segal, Jeffrey A., see Epstein, Lee 845
Seligmann, Linda J. *Between Reform and Revolution: Political Struggles in the Peruvian Andes, 1969–1991.* Reviewed by Cynthia McClintock 462
Serbin, Andrés, see McCoy, Jennifer 219
Sexton, Terri A., see O'Sullivan, Arthur 198
Seyd, Patrick, see Whitely, Paul 227
Shafer, Byron E., and William J. M. Claggett. *The Two Majorities: The Issue Context of Modern American Politics.* Reviewed by Eric R. A. N. Smith 438
Shafir, Gershon. *Immigrants and Nationalists: Ethnic Conflict and Accommodation in Catalonia, the Basque Country, Latvia, and Estonia.* Reviewed by Milton J. Esman 682
Shambaugh, David, ed. *Deng Xiaoping: Portrait of a Chinese Statesman.* Reviewed by Quansheng Zhao 683
Shamir, Ronen. *Managing Legal Uncertainty: Elite Lawyers in the New Deal.* Reviewed by Mark Kessler 922
Shapiro, Ian, and Judith Wagner DeCew, eds. *Theory and Practice: Nomos XXXVII.* Reviewed by John G. Gunnell 413
Sheffrin, Steven M., see O'Sullivan, Arthur 198
Shih, Chih-Yu. *State and Society in China's Political Economy.* Reviewed by Yasheng Huang 944
Shiratori, Rei, see Alexander, Herbert E. 208
Shively, W. Phillips, see Achen, Christopher H. 399
Silver, Christopher, and John V. Moeser. *Separate City: Black Communities in the Urban South.* Reviewed by Paula D. McClain 867
Simon, Julia. *Mass Enlightenment: Critical Studies in Rousseau and Diderot.* Reviewed by Tracy B. Strong 897
Sisk, Timothy M. *Democratization in South Africa: The Elusive Social Contract.* Reviewed by Timothy Shaw 226
Smith, Christopher E., see Schultz, David A. 921
Smith, Michael Peter, and Joe R. Feagin. *The Bubbling Cauldron: Race, Ethnicity, and the Urban Crisis.* Reviewed by Paula D. McClain 867
Smith, Renée M., see Box-Steffensmeier, Janet M. 567
Smith, Robert C. *We Have No Leaders: African Americans in the Post-Civil Rights Era.* Reviewed by Paula D. McClain 867
Smith, Tara. *Moral Rights and Political Freedom.* Reviewed by Jack Donnelly 898
Smith, William C., see McCoy, Jennifer 219
Smolla, Rodney A., ed. *A Year in the Life of the Supreme Court.* Reviewed by David M. O'Brien 204
Snider, Don M., and Miranda A. Carlton-Carew, eds. *U.S. Civil-Military Relations: In Crisis or Transition?* Reviewed by Sam C. Sarkesian 706
Snyder, James M., Jr., see Groseclose, Tim 303
Soder, Roger, ed. *Democracy, Education, and the Schools.* Reviewed by William A. Galston 660
Sorenson, Leonard R. *Madison on the "General Welfare" of America: His Consistent Constitutional Vision.* Reviewed by George W. Carey 923
Spaeth, Harold J., see Brenner, Saul 418
Spicer, Michael W. *The Founders, the Constitution, and Public Administration: A Conflict in World Views.* Reviewed by Donald J. Maletz 205
Sprague, John, see Huckfeldt, Robert 192
Stam, Allan C., III, see Bennett, D. Scott 239
Stamato, Linda, see Vasquez, John A. 471
Stambouli, Andrés, see McCoy, Jennifer 219
Stein, Michael B., see Easton, David 171
Stein, Robert M., and Kenneth N. Bickers. *Perpetuating the Pork Barrel: Policy Subsystems and American Democracy.* Reviewed by Gerald Strom 661
Stoker, Gerry, see Judge, David 433
Sullivan, John L., see Marcus, George E. 916
Summy, Ralph, and Michael E. Salla, eds. *Why the Cold War Ended: A Range of Interpretations.* Reviewed by George H. Quester 471

Sunderland, Lane V. *Popular Government and the Supreme Court: Securing the Public Good and Private Rights.* Reviewed by Gary Jeffrey Jacobsohn ... 924

Tarrow, Sidney. "Making Social Science Work across Space and Time: A Critical Reflection on Robert Putnam's *Making Democracy Work.*" ... 389

Tarrow, Sidney. "Social Movements in Contentious Politics: A Review Article." ... 874

Tatalovich, Raymond. *Nativism Reborn? The Official English Language Movement and the American States.* Reviewed by Ronald Schmidt, Sr. ... 439

Tate, C. Neal, and Torbjörn Vallinder, eds. *The Global Expansion of Judicial Power.* Reviewed by Jerry Waltman ... 684

Tatum, Jesse S. *Energy Possibilities: Rethinking Alternatives and the Choice-Making Process.* Reviewed by Robert M. Lawrence ... 440

Taylor, Bron Raymond, ed. *Ecological Resistance Movements: The Global Emergence of Radical and Popular Environmentalism.* Reviewed by Robert Paehlke ... 957

Telhami, Shibley, see Esman, Milton J. ... 694

Terriff, Terry. *The Nixon Administration and the Making of U.S. Nuclear Strategy.* Reviewed by Eric Mlyn ... 707

Thain, Colin, and Maurice Wright. *The Treasury and Whitehall: The Planning and Control of Public Expenditures, 1976–1993.* Reviewed by Paul Whiteley ... 945

Theiss-Morse, Elizabeth, see Marcus, George E. ... 916

Thiele, Leslie Paul. *Timely Meditations: Martin Heidegger and Postmodern Politics.* Reviewed by Hans Sluga ... 180

Thompson, Dennis F. *Ethics in Congress: From Individual to Institutional Corruption.* Reviewed by Charles Stewart III ... 206

Thompson, Mark R. *The Anti-Marcos Struggle: Personalistic Rule and Democratic Transition in the Philippines.* Reviewed by Gretchen Casper ... 946

Thompson, Norma. *Herodotus and the Origins of the Political Community: Arion's Leap.* Reviewed by Clifford Orwin ... 899

Thurber, James A., and Roger H. Davidson, eds. *Remaking Congress: Change and Stability in the 1990s.* Reviewed by Paul J. Quirk ... 925

Tilly, Charles. *Popular Contention in Great Britain, 1758–1834.* Reviewed by Sidney Tarrow ... 874

Tismaneanu, Vladimir, ed. *Political Culture and Civil Society in Russia and the New States of Eurasia.* Reviewed by Russell Bova ... 685

Torpey, John C. *Intellectuals, Socialism and Dissent: The East German Opposition and Its Legacy.* Reviewed by Vladimir Tismaneanu ... 947

Traugott, Michael W., see Lavrakas, Paul J. ... 654

Tsebelis, George. "More on the European Parliament as a Conditional Agenda Setter: Response to Moser." ... 839

Tully, James, ed. *Philosophy in an Age of Pluralism: The Philosophy of Charles Taylor in Question.* Reviewed by William E. Connolly ... 181

Turner, Patricia A., see MacManus, Susan A. ... 915

Vallinder, Torbjörn, see Tate, C. Neal ... 684

Van Beek, Stephen D. *Post-Passage Politics: Bicameral Resolution in Congress.* Reviewed by Walter J. Oleszek ... 662

Van Cott, Donna Lee, ed. *Indigenous Peoples and Democracy in Latin America.* Reviewed by Eric Selbin ... 686

Van Parijs, Phillipe. *Real Freedom for All: What (If Anything) Can Justify Capitalism?* Reviewed by Howard J. Sherman ... 900

Vasquez, John A., James Turner Johnson, Sanford Jaffe, and Linda Stamato, eds. *Beyond Confrontation: Learning Conflict Resolution in the Post–Cold War Era.* Reviewed by Marc Howard Ross ... 471

Verba, Sidney. "The Citizen Respondent: Sample Surveys and American Democracy." ... 1

Villa, Dana R. *Arendt and Heidegger: The Fate of the Political.* Reviewed by Fred Dallmayr ... 901

Viroli, Maurizio. *For Love of Country: An Essay on Patriotism and Nationalism.* Reviewed by David Miller ... 885

Vogel, David. *Trading Up: Consumer and Environmental Regulation in a Global Economy.* Reviewed by Lynton K. Caldwell ... 472

Volgyes, Ivan, and Zoltan Barany, eds. *The Legacies of Communism in Eastern Europe.* Reviewed by Michael Bernhard ... 687

von Mettenheim, Kurt. *The Brazilian Voter: Mass Politics in Democratic Transition, 1974–1986.* Reviewed by Anthony W. Pereira ... 688

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Wahlbeck, Paul, see Maltzman, Forrest ... 581

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Wegener, Bernard, see Kluegel, James R. ... 676

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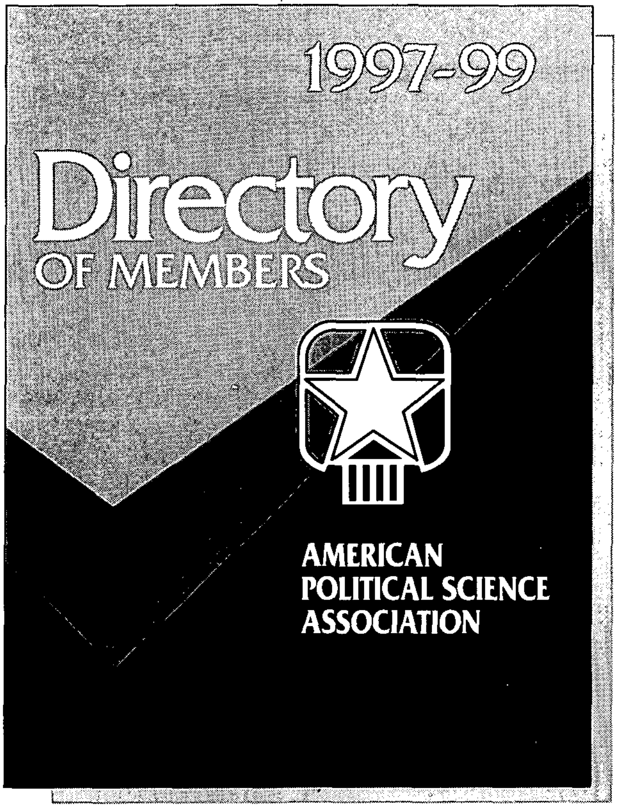

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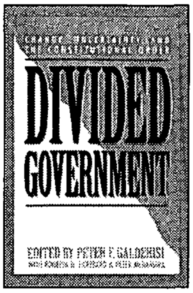

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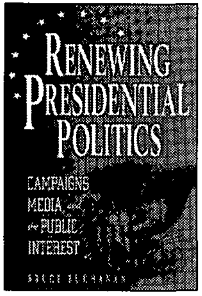
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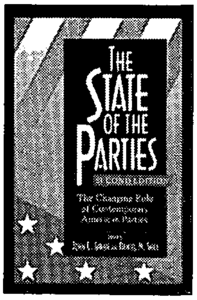

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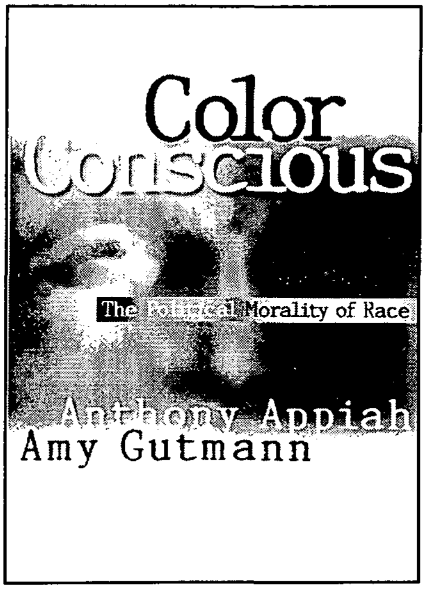

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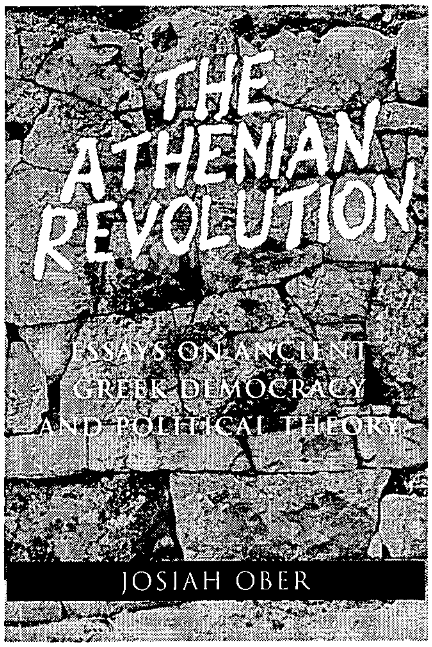

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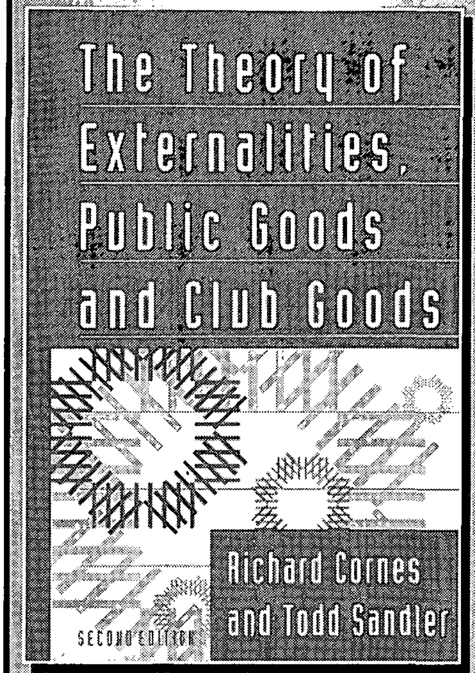
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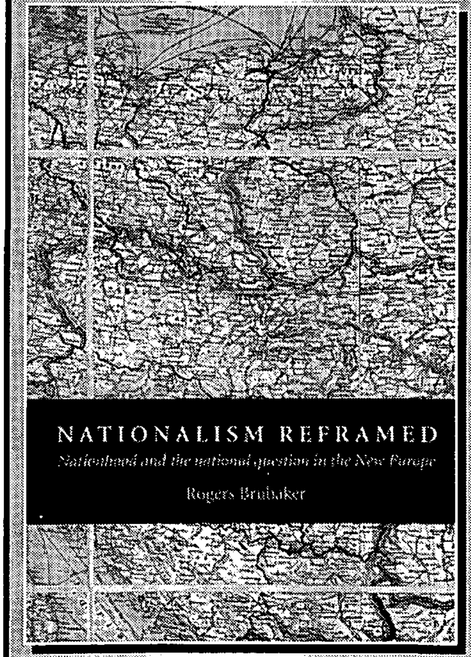

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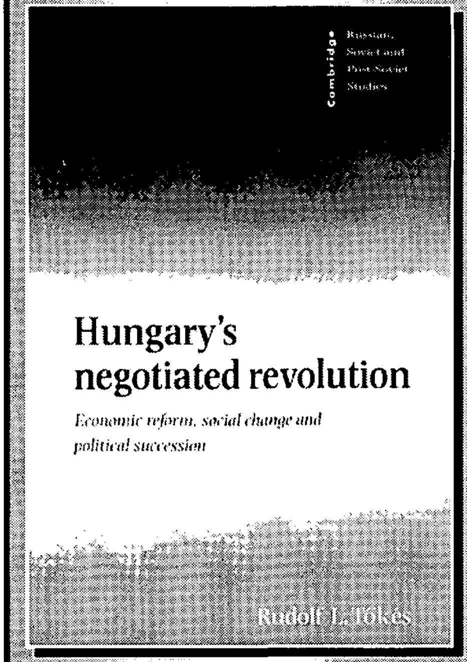
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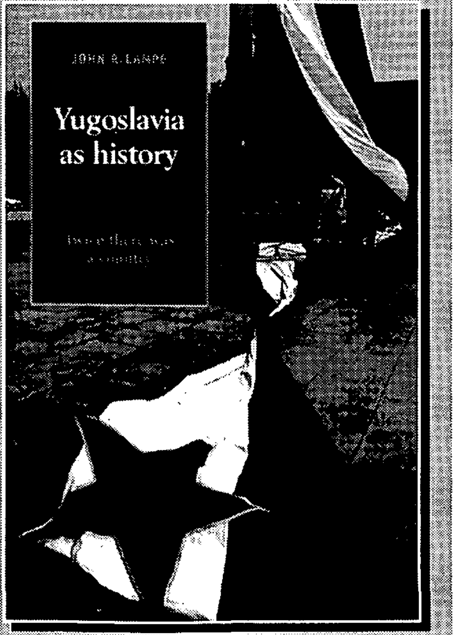

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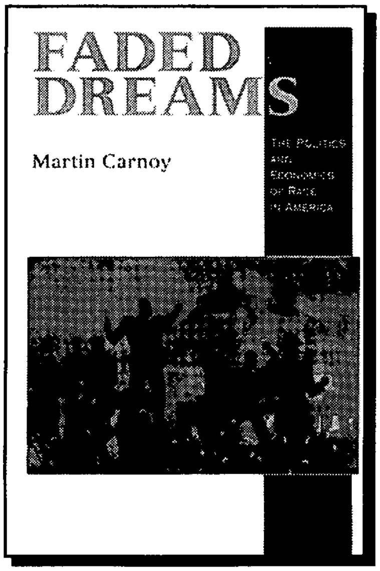

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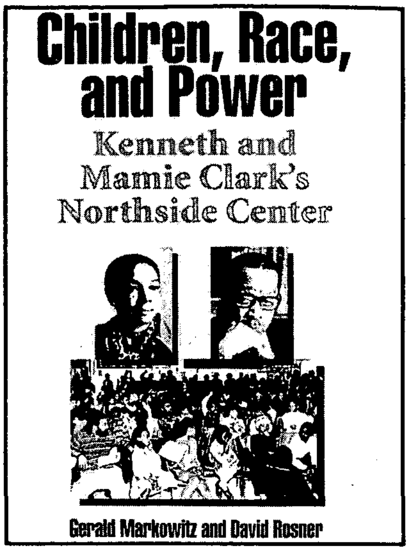
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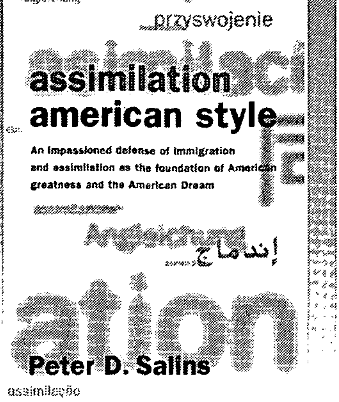
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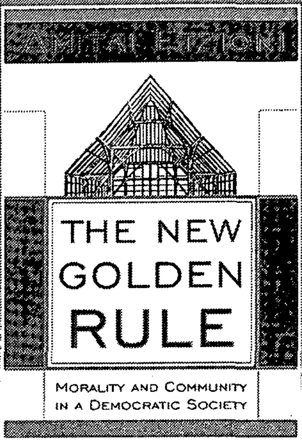

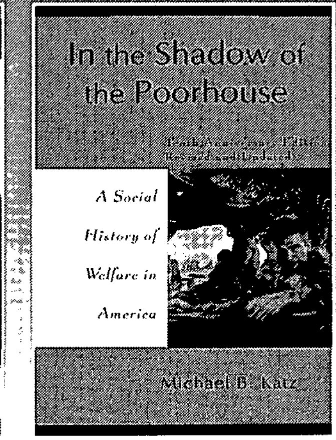
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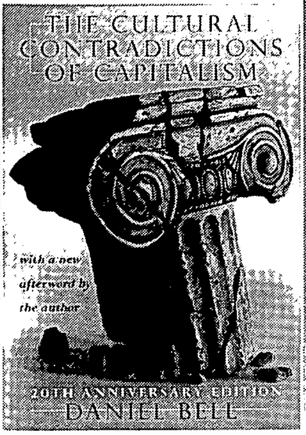
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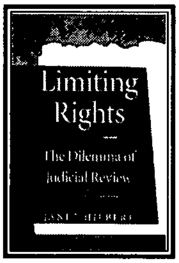
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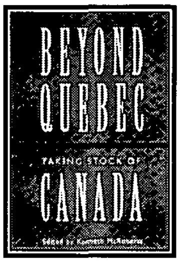
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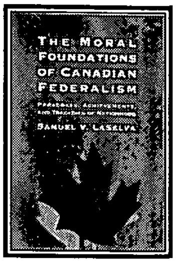
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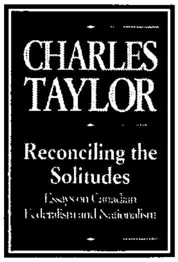
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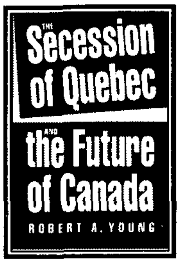
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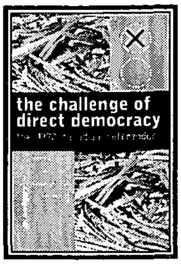
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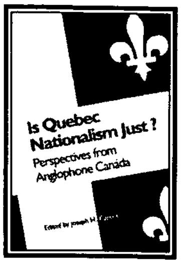
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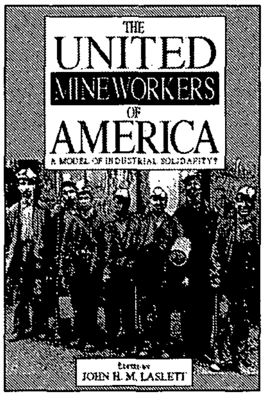

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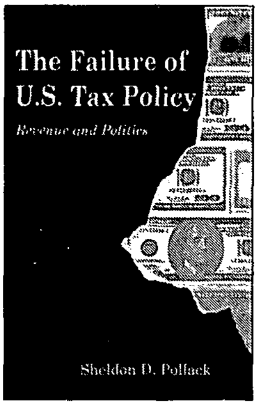

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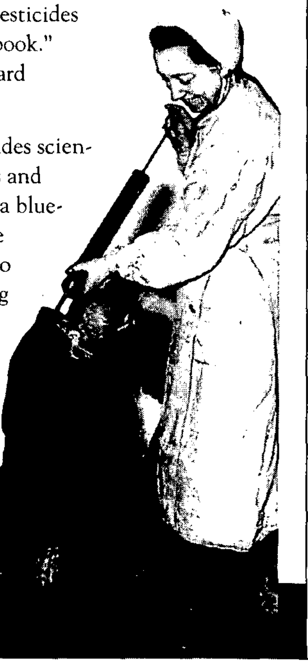

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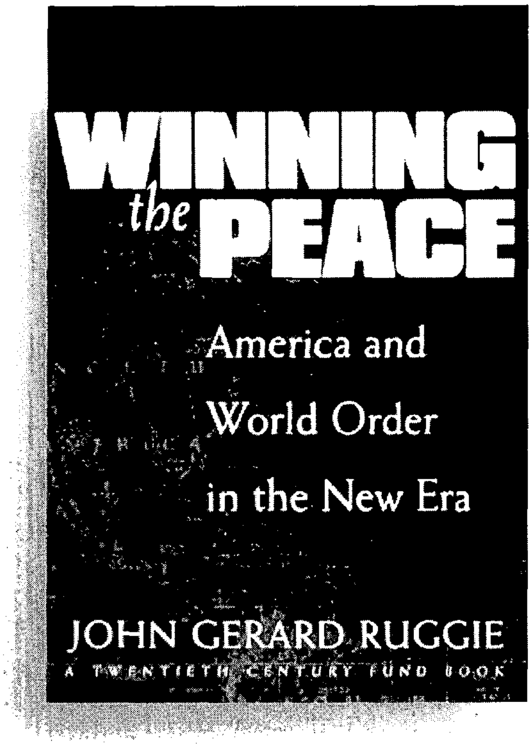

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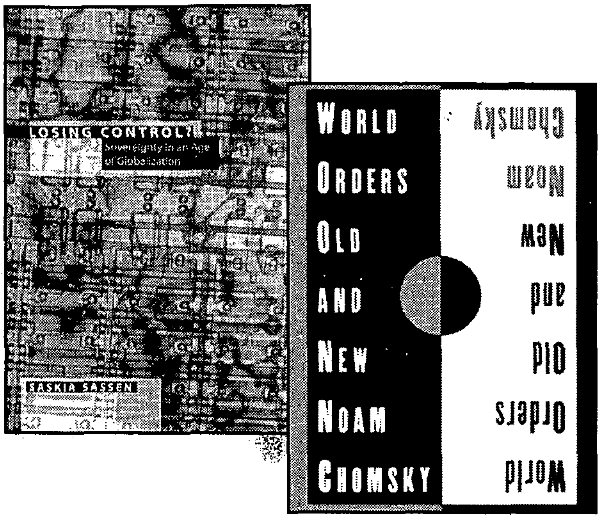

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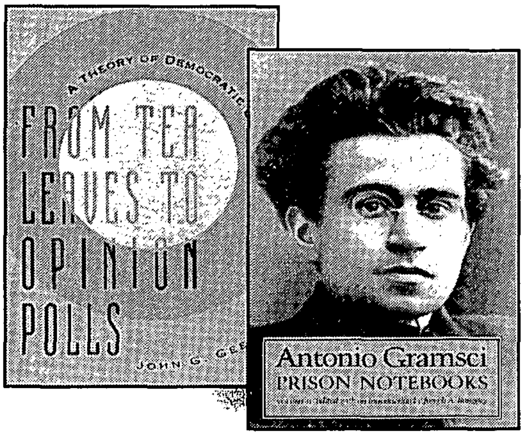

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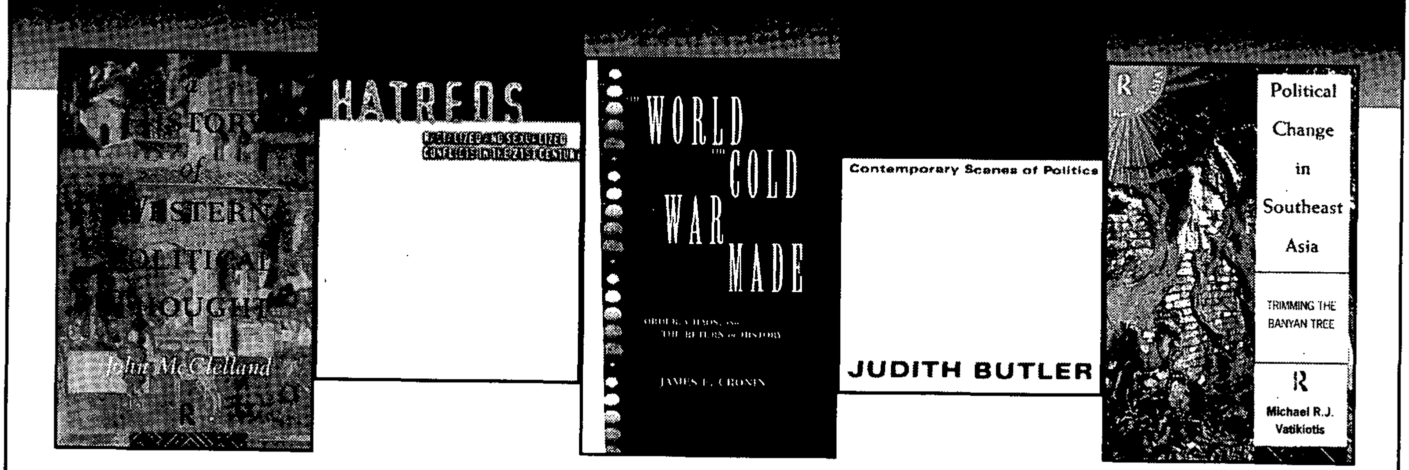
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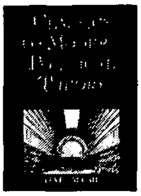

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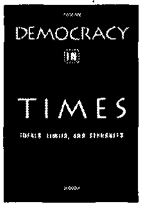

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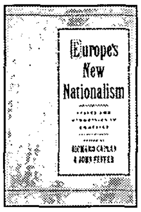

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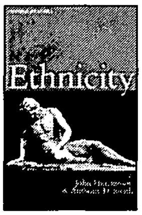

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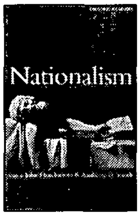

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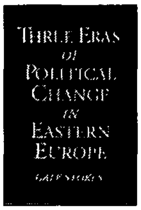

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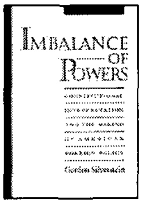

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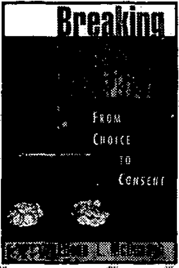

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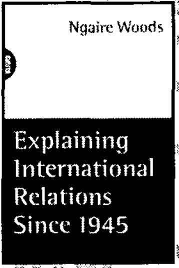

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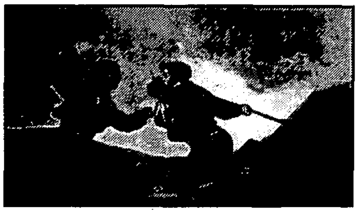

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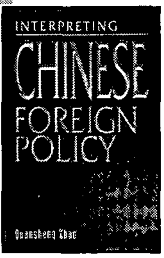

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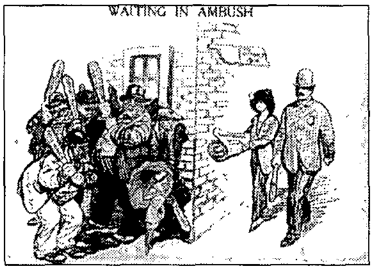

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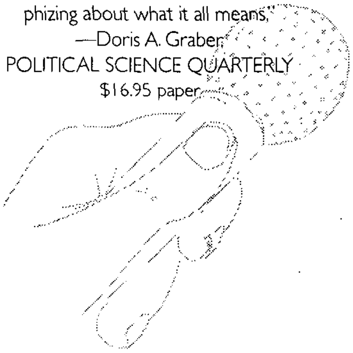

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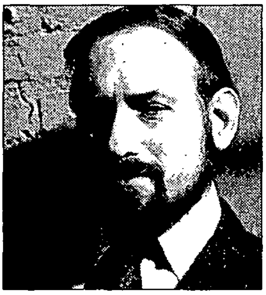

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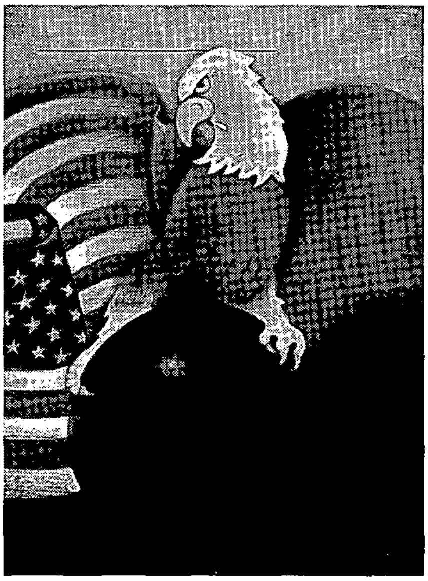

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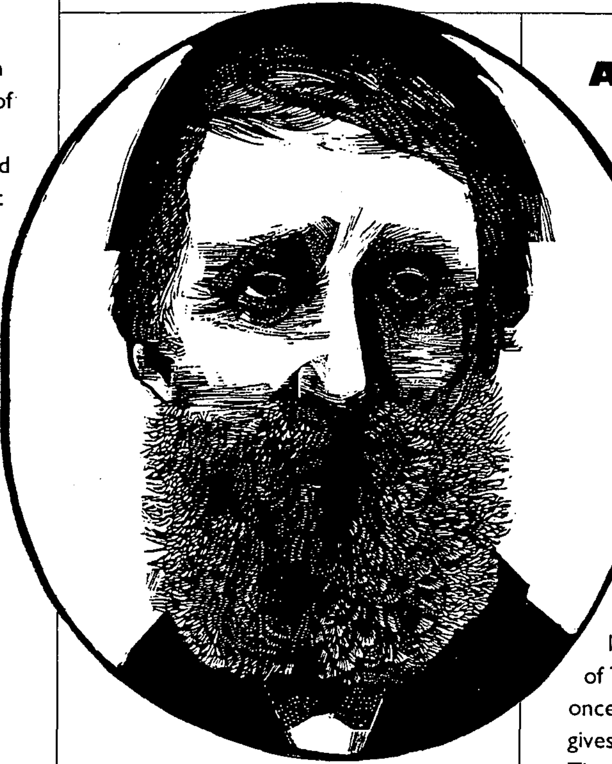

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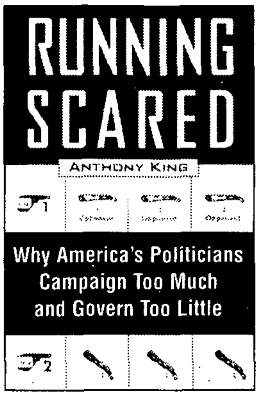
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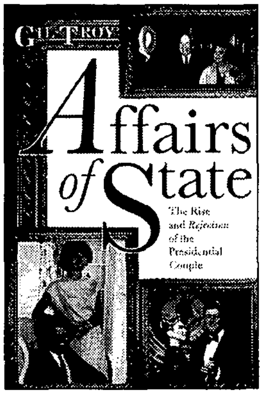

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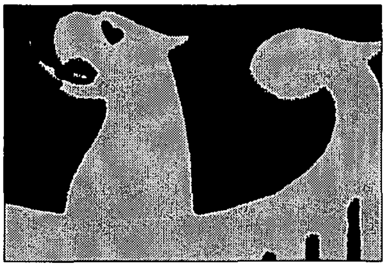

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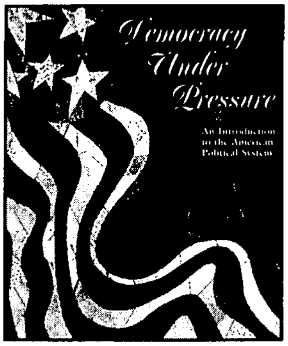

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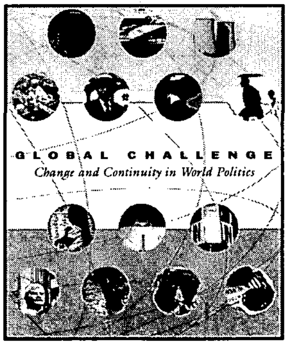

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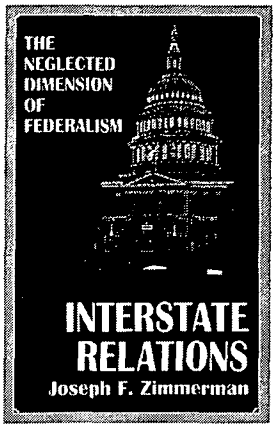

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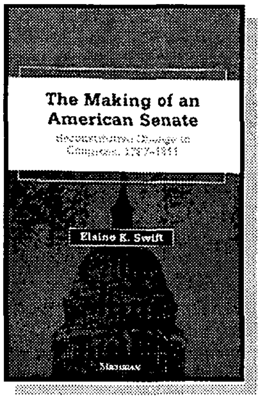

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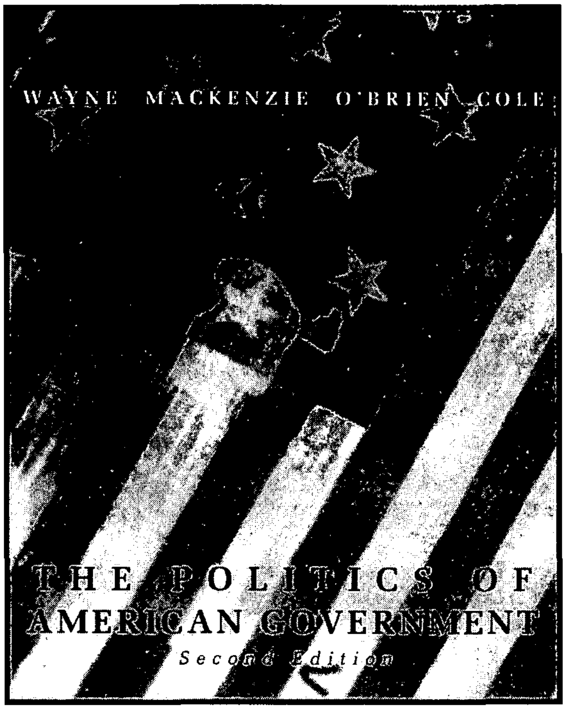
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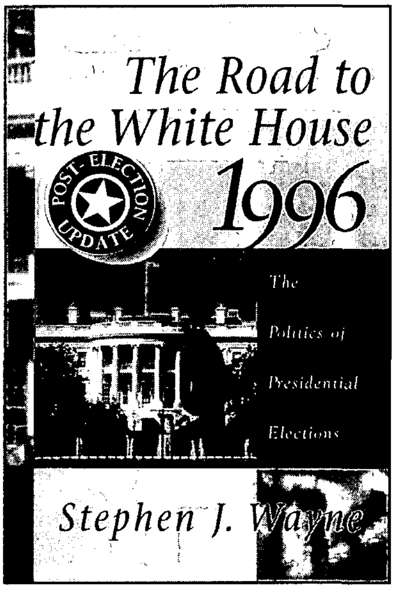
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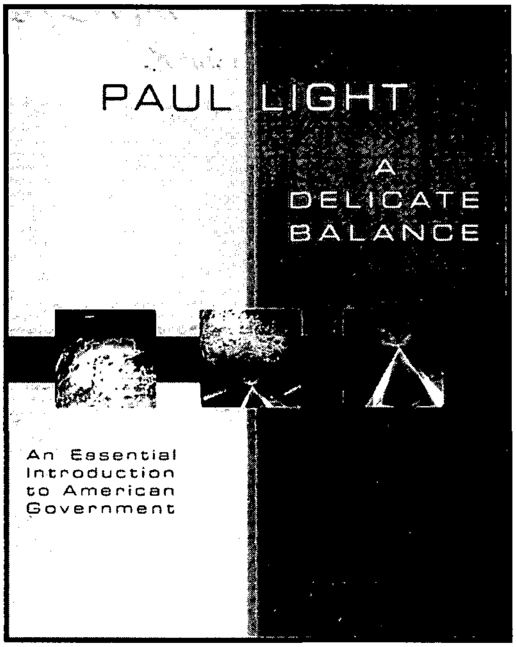
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Table of Contents

EDITOR'S NOTES viii

ARTICLES

Explaining Interethnic Cooperation
James D. Fearon and David D. Laitin 715

The Electoral Connection in the Chinese Countryside
Melanie Manion 736

Uncertainty, Shifting Power, and Appeasement
Robert Powell 749

Do Bills of Rights Matter? The Canadian Charter of Rights and Freedoms
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Partisan Effects of Voter Turnout in Senatorial and Gubernatorial Elections
Jack H. Nagel and John E. McNulty 780

Do Majority–Minority Districts Maximize Substantive Black Representation in Congress?
Charles Cameron, David Epstein, and Sharyn O'Halloran 794

Voting and Vetoing
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Michael J. Webber and G. William Domhoff 824

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Peter Moser 834

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George Tsebelis 839

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Lee Epstein, Jeffrey A. Segal, and Timothy Johnson 845

Issues, Agendas, and Decision Making on the Supreme Court
Kevin T. McGuire and Barbara Palmer 853

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Black Politics at the Crossroads? Or in the Cross-Hairs?
Paula D. McClain 867

Social Movements in Contentious Politics: A Review Article
Sidney Tarrow 874

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Political Theory 884
American Politics 904
Comparative Politics 928
International Relations 952